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Timely justice as a determinant of economic growth☆,☆☆

Simeon Djankov [a],*, Alessandro Melcarne [b], Giovanni B. Ramello [c], Rok Spruk [d]

[a] Financial Markets Group, London School of Economics, United Kingdom
[b] Department of Private Law and Critique of Law, University of Padova, Italy
[c] ESt, University of Torino, Italy
[d] Faculty of Economics, University of Ljubljana, Slovenia

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ABSTRACT

We investigate how timeliness in enforcing legal contracts affects economic growth across countries. We focus on judicial timeliness as a proxy for courts' performance in a large panel of 169 countries over the 2004–2019 period. We show that, by raising uncertainty and promoting opportunistic behaviors in business transactions, slower courts hinder economic development. The relationship is robust to diverse model specifications and appears stronger for business environments more heavily relying on judiciaries such as economies undergoing rapid growth, countries characterized by low human capital and civil law jurisdictions.

1. Introduction

Economists widely agree that proper enforcement of contracts is a necessary condition for economies to prosper (Montesquieu, 1748; Smith, 1776; Weber, 1905; von Hayek, 1973; Glaeser et al., 2004; Jayachandran and Kremer, 2006; Gennaioli et al., 2022). The way the law is enforced is a key element of the institutional system governing society and the economy (Sala-i Martin et al., 2004); and this task is ultimately performed by judiciaries. In this study we investigate the link between the functioning of the judiciary and economic growth. After going over the theoretical reasons why a good judiciary ought to benefit economic development, we perform empirical analyses exploiting both temporal and spatial variation in a cross-country dataset of 169 nations.

We propose a number of extensions to the extant literature. First, although several works examine the contribution of judicial timeliness to cross-country firm productivity (Chemin, 2020) and aggregate economic growth for the European Union (Kapopoulos and Rizos, 2024), no previous study has attempted to examine the underlying relationship both across countries and over time in a worldwide panel. To tackle the endogeneity problems that commonly affect cross-country studies seeking to show the effect of institutions on economic performance, we apply advanced econometric methods to motivate the robustness of our results. Previous studies have already identified the impact of judicial timeliness on the economy (Chemin, 2009a; Kondylis and Stein, 2023; Ichino et al., 2023; Chemin et al., 2023). It is useful to supply further comparative analysis of how judicial systems' functioning contributes to growth, explaining the variance in economic prosperity across the world, as in Chemin (2021).

Second, we specifically look for evidence on the relationship between judicial performance and economic growth in countries with more court-dependent business environments. The previous literature suggests that in countries with high growth, low human capital and of civil law origin the court system is more relevant in resolving commercial disputes. Economies experiencing higher

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* Corresponding author.
E-mail address: S.Djankov@lse.ac.uk (S. Djankov).

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rates of growth, more differentiated product sophistication, and higher levels of per capita income are usually characterized by impersonal and high frequency economic transactions for which relational contracting is not as well equipped as a formal judiciary (Williamson, 1985; Johnson et al., 2002; Eisenberg et al., 2013). Alternative dispute resolution mechanisms that avoid the shortcomings of some judiciaries poor performances are less likely to work in societies affected by low levels of human capital (Bosio et al., 2022). Finally, jurisdictions belonging to the Civil Law legal family are usually characterized by higher levels of procedural complexity that reflect lower levels of trust (Aghion et al., 2010), thus making judicial institutions more relevant, but also litigating more cumbersome (La Porta et al., 1998; Djankov et al., 2003b).

We exploit variation in judicial timeliness across 169 countries for the period 2004–2019 to examine the contribution of judicial timeliness to economic growth and estimate the structural parameters consistently. We estimate a series of dynamic panel-level specifications and robustness-related variants to find evidence of a statistically significant and consistently positive relation between judicial timeliness and economic growth. Our estimates suggest that 100 extra days needed to solve a case (about one-third of a standard deviation) is associated with a GDP decrease of about 1.2 percentage points. This relation is not uniform among subsamples of countries: countries in which economic transactions rely more on judicial enforcement are consistently more heavily affected by this relation. Nations with elevated growth trajectories are impacted 73% more than the average economy. The hypothesized relationship under scrutiny is equally 47% stronger in countries with low human capital. Civil law jurisdictions are affected 33% more than in the full sample.

The remainder of the paper is structured as follows. Section 2 presents the theoretical framework with respect to the importance of enforcing institutions – and more specifically the timeliness of justice – for economic growth. Sections 3 and 4 describe, respectively the data employed and our empirical strategy. Section 5 presents our estimates, while in Section 6 our results undergo several robustness checks. Conclusions are drawn in Section 7.

2. Courts and economic growth

Contracts need to be enforced in order to foster economic performance (Weber, 1905; von Hayek, 1973). When it comes to explaining the economic success of countries via legal determinants, the literature has focused on how the historical evolution of legal systems (for example, common law vs. civil law) might explain today's variance in countries' prosperity. Proponents of this view have argued that the evolutionary nature of judge-made-law, attributed to common law systems, allows legal institutions to better adapt to changes in societies (von Hayek, 1960), and also that common law systems supply a better environment for the development of private markets (La Porta et al., 1998; Djankov et al., 2002).

These results do not take into account the substantial differences in economic development that exist today among countries sharing the same legal tradition. Among countries belonging to the common law tradition we find at the same time the United States and the United Kingdom, but also Bangladesh and Ghana. If institutions are "humanly devised constraints" (North, 1990), their effectiveness depends on how they are implemented (Bosio et al., 2022).

But to what extent do judiciaries affect the economy? As markets become more complex, impersonal trade in economic transactions with many business partners takes the place of repeated dealings, in which reputation alone might be sufficient to make opportunistic behaviors not remunerative. As we move away from repeated games to "one-shot" interactions, impersonal exchange must be accompanied by some sort of "third-party" enforcement that lowers the transaction costs arising from the uncertainty in performance of contractual obligations (North, 1990). Trading partners usually encourage suppliers/creditors to undertake investments by writing long-term contracts. In a world of incomplete contracts (Grossman and Hart, 1986), once investment costs are sunk, there is an immediate incentive to renege on contractual obligations and try to capture trading partners' rents. On the other hand, if transaction costs associated to searching for new business partners are high, suppliers will try to use their monopoly power and impose higher prices (Chemin, 2009b). Judiciaries play an important role in limiting opportunistic behavior, by reducing uncertainty in economic interactions.

A well-functioning judiciary acts as an important deterrent against economic agents' willingness to deviate from previously signed contracts (Williamson, 1985). Not only would transactions costs thus rise in the event of an "imperfect" judiciary, but financing opportunities would likewise be constrained. A deficient law enforcement will incentivize opportunism on borrowers' side: Anticipating the difficulty that creditors face when recovering their loans, debtors will be more incentivized to default. However, a vicious circle would push creditors to anticipate borrowers' opportunistic behavior and consequently reduce the availability of credit (Jappelli et al., 2005) or increase interest rates (Visaria, 2009). Post-contractual opportunism incentivized by judicial institutions' poor performances also affects firms' employment decisions (Ichino et al., 2023) and firms' dynamics (Melcarne and Ramello, 2020).

3. Descriptive statistics

A description of all variables and the corresponding sources is seen in Table 1. Our dependent variable is the growth rate of GDP calculated through the output-related method, obtained from the recent edition of Penn World Tables (Feenstra et al., 2015). The independent variable is the judicial timeliness indicator established by World Bank. The vector of control variables includes (i) the investment share of GDP, (ii) the level of government revenue as a share of GDP, (iii) population size, (iv) population growth, (v) indicator of exchange-rate distortions, and (vi) trade openness measured as a share of exports and imports over GDP. All control variables are collected from the recent edition of Penn World Tables. Our sample comprises a balanced panel of 169 countries for the period 2004–2019 which yields a sample of 2704 country-year observation pairs.

Table 1
Descriptive statistics.

	Obs	Mean	Std. Dev.	Min	Max
Panel A: Outcome variables					
Growth rate of GDP (output-side)	2535	0.042	0.097	−1.604 (Venezuela)	0.601 (Antigua and Barbuda)
Panel B: Key treatment variable					
Judicial timeliness	2704	642.01	303.01	120 (Singapore)	1785 (Guinea Bissau)
Panel C: Structural covariates					
Investment GDP share	2704	0.232	0.089	−0.029 (Venezuela)	0.925 (Djibouti)
Government size	2704	0.184	0.075	0.014 (Haiti)	0.751 (São Tomé and Principe)
Trade openness	2704	−0.077	0.189	−2.096 (Antigua and Barbuda)	0.757 (Azerbaijan)
Exchange rate distortions	2704	0.009	0.097	0 (Portugal)	1 (Syria)
Population growth	2535	0.014	0.015	−0.045 (Syria)	0.175 (Qatar)
Population size (log)	2704	2.041	1.895	−3.072 (St. Kitts and Nevis)	7.268 (China)

All variables in Panel A and C are extracted from Feenstra et al. (2015). Judicial timeliness's methodology can be found in Djankov et al. (2003b).

Among our subsamples of interest, judicial timeliness varies to different extents. Civil law countries tend to have slightly faster judiciaries than their common law counterparts (640 *vs.* 649 days). More intuitively, nations characterized by higher growth rate (620 days) and higher human capital (563 days) enjoy substantially more timely courts than low growth (665 days) or low human capital (681 days) countries.

3.1. Judicial timeliness

Different measures related to judicial performance have been used in the scholarly literature. We select as our main measure judicial timeliness (JT), proxied by the time taken by courts to dispose over a typical contract default situation. Concentrating on a specific institutional dimension such as judicial timeliness has some clear advantages when it comes to identifying the relationship between the judiciary and economic performance. First of all, timeliness is a fairly stable indicator of judicial performance. Substantial changes in judicial performance are the outcome of reforms affecting the law, the judiciary's organization, or general litigation habits (Melcarne and Ramello, 2015). While significant variations within jurisdictions surely exist as judges might differ in their decision making process even when facing observably similar cases (Chan et al., 2022), our timeliness metric measures year-country averages.[1] This variable was created by a survey methodology submitted to a large pool of cooperating law firms in every country of the dataset (Djankov et al., 2003b). Although survey data might present limitations connected to hypothetical answers, these limitations supply a unique vantage point. Since the respondents are professionals who on a daily basis solve these kinds of cases within their national judicial system, their answers are grounded in everyday experience. In addition, the peculiarity of this dataset helps to overcome an opposite critique that is raised about data coming from enterprise surveys: that they might capture local characters which affect the overall comparability. In this respect, the uniform construction of the questionnaire enhances the comparability of results across countries. Also, in comparison with other available datasets, this sample is wider in terms of number of countries and longitude. It is no coincidence that this dataset has been used by many scholars, including recently by Bosio et al. (2022), Kalkschmied (2023) and Bosio (2024). Later we test the robustness of our estimates on a subsample of countries for which we were able to obtain administrative data concerning judicial timeliness.

If the nature of JT data yields advantages in terms of institutional measurement, as discussed above, on the other hand it might pose a problem concerning the quantity-quality trade-off. Timeliness does not necessarily capture the "qualitative" dimension of judges' work. There might be a trade-off between judicial performance in its quantitative dimension (as we measure timeliness) and the intrinsic quality of justice as delivered by courts. For example, the necessity of respecting procedural hurdles and other physical constraints necessarily slows down the work of courts (Chemin, 2012). In such a case, reducing time might be detrimental to the economy. That said, the available empirical evidence shows, at best, no relationship between judicial timeliness and a country's quality of justice (Bray et al., 2016; Coviello et al., 2019; Melcarne et al., 2021).

[1] It should be noted that our measure of timeliness captures the duration of disputes and excludes the costs of the enforcement. Although the consideration of cost has clear and indisputable advantages compared to the time indicator only, it also more closely captures the degree of access to justice which is a different dimension of judicial effectiveness from our key variable of interest.

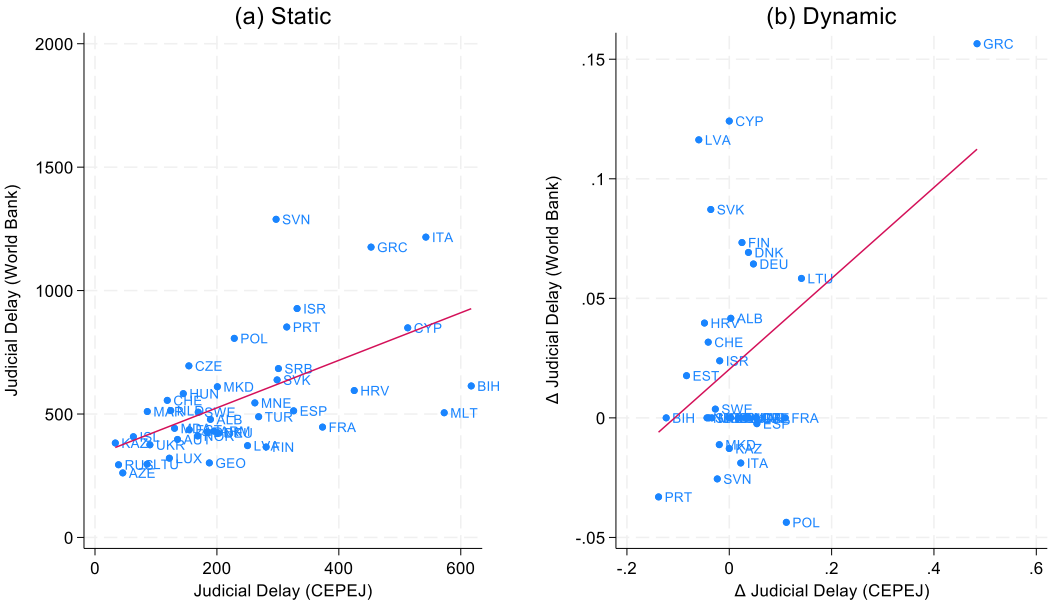

Fig. 1. Average disposition time in civil and commercial litigious cases and judicial timeliness across 41 countries, 2004–2019.

As emphasized above, the source of our courts' effectiveness variable has both advantages and drawbacks. However, an administrative source-JT metric covering the same pool of countries across the same time span does not exist. A viable second-best alternative might be found in the reports published by the European Commission for the Efficiency of Justice (CEPEJ). The CEPEJ has the merit of collecting administrative data on all relevant characteristics of judicial systems, including the length of judicial procedures, from the Ministries of Justice's statistical offices of its 47 member states. In addition to a reduced sample size, this dataset presents two further shortcomings. First, data collection is biannual. Second, not every country consistently reports all statistics on every report, thus making the panel unbalanced.[2]

Two noteworthy insights can be drawn from this particular comparison. First, the time to disposition varies widely across countries in a similar manner to the World Bank's judicial timeliness variable. For instance, the disposition time in our sample varies from a mere 13 days in the Russian Federation to 849 days in Malta. And second, countries with a lengthy disposition times such as Malta and Bosnia and Herzegovina tend to experience a gradual reduction in disposition times, improving the effectiveness of their judiciary over time. Others such as Greece, France and Italy have tended to undergo a deterioration of disposition time up to the present day, whilst elsewhere the length of disposition has remained relatively stable over time. Against this backdrop, Fig. 1 compares, both statically and dynamically, the World Bank's judicial timeliness variable to the CEPEJ's average disposition time variable. In Panel (a) the variables exhibit a high correlation coefficient of +0.55 and is statistically significant at 1% (*i.e.*, *p*-value = 0.000). A potential critique could emphasize "dynamic inconsistency", implying that over time correlation between CEPEJ and WB measures wanes due to issues in reporting. If this critique was true, then average change in WB delay variable should not predict average changes in CEPEJ variable and vice versa, and should yield a low correlation. Panel (b) reports average changes (geometric mean) in CEPEJ measure against average changes in WB measure of delay. Correlation coefficient between both variable is +0.39 and is statistically significant at 5% (i.e. *p*-value = 0.027). Since the short time to disposition in several countries in our sample (*i.e.*, Belarus, Russia, Azerbaijan to name a few) may be explained by executive subordination of the judiciary that may result in artificially lowered disposition time, the correlation coefficient remains strong once the potentially outlying observations are excluded from the comparison, both in terms of coefficient size and significance.

We replicated our baseline cross-country panel-level growth regressions by replacing the judicial timeliness variable with the CEPEJ disposition time variable whilst retaining the full set of confounders. Table 10 in the Online Appendix A reports our estimates. Evidence confirms our prior estimates, suggesting that reducing the disposition time by one standard deviation (*i.e.*, roughly 158 days) is associated with a 0.1 increase in annual growth rate. The estimated coefficient is statistically significant at 1% and does not deviate much once the standard errors are clustered on both country and yearly dimension.

4. Empirical strategy

To ensure the robustness of our findings from endogeneity issues, we adopt a two-step empirical approach. First, we estimate a baseline model that incorporates country-fixed effects and standard controls to address potential omitted variable bias arising from

[2] The data on judicial delays in European countries can be found here. We use data for 38 countries extracted from Reports dating from 2006 through 2014, for a total of 5 waves.

unobservable common-cause confounders. We include in our econometric specifications several widely accepted determinants of economic growth as, for example, the level of investments as a share of GDP (Barro, 1991; Barro and Sala-I-Martin, 1992; Levine and Renelt, 1992; Sala-i Martin et al., 2004), that could at the same time influence our regressor of interest, as higher investments may correlate with better performances of the judiciary. We also recognize the concern that some of these controls may themselves be endogenous to judicial timeliness. To address this, we also treat our controls as endogenous regressors in our dynamic panel models. However, recognizing that endogeneity concerns might persist, we refine our analysis by implementing a panel vector-autoregressive (PVAR) model. This second step introduces a more sophisticated econometric structure that explicitly accounts for dynamic interdependencies and potential reverse causality between judicial timeliness and economic growth. It is not implausible to expect longer judicial delays in the occurrence of an economic downturn, with many defaults engulfing and thus slowing down courts. By sequentially building on the initial estimation with a more comprehensive methodology, we thus strengthen the validity of our results and provide a more rigorous assessment of the relationship under investigation.[3]

4.1. Baseline model

Our approach to estimate the relationship between judicial timeliness and economic growth is grounded in panel-level estimation, particularly dynamic panel estimators and panel vector autoregression analysis. Five specific advantages emanate from panel-level analysis that motivates our choice of estimators. First, since economic growth is a dynamic process, contemporary rates of growth often depend on their past realization. By making use of lagged rates of growth, the inherent persistence in growth dynamics is directly captured through the dynamic panel estimator which further reduces the bias arising from excluding relevant variables correlated with both our explanatory variable of interest and our dependent variable. Second, judicial timeliness and economic growth are endogenously determined through a simultaneous process. Shorter delays may boost economic growth inasmuch as higher rates of growth may reduce the demand for litigation. Furthermore, business cycle downturns may cause higher caseload and slower judicial decisions in a similar vein as reduced caseload and faster judicial decision may influence the aggregate short-run fluctuations and long-term economic growth. To this end, we take a unique advantage of dynamic panel estimators by exploiting lagged rates of growth and judicial delays to unravel potentially reverse causation. Third, dynamic panel estimator allow us to better handle unobserved heterogeneity than alternative empirical strategies. For instance, countries differ in several dimensions that are not directly observable such as cultural factors and the historical legacies. By explicitly incorporating country-fixed effect, these unobserved characteristics are directly controlled for, which might otherwise lead to a biased estimate. Fourth, measurement errors in the reported judicial timeliness variables can be quite common. However, dynamic panel estimator largely mitigate the impact of these errors by using internal instruments which tend to be less affected by measurement noise. And lastly, the temporal dimension in cross-country panels is often short relative to the number of countries, which is colloquially known as small t and large n setting. Dynamic panel estimators are designed and particularly well suited for such settings and provide more reliable estimates compared to traditional time-series approach that may fail. By making use of past realization of growth rates and judicial timeliness, the reverse causality in the underlying relationship can be directly tested. In Sections 5.2 and 5.3 we use estimate panel models in which we instrument endogenous variables using their own lagged levels and differences within the system GMM framework, applying a one-year lag as the starting point to avoid contemporaneous feedback. This approach exploits the orthogonality conditions between past realizations of these variables and the current first-differenced error term, We estimate both difference GMM (Arellano and Bond, 1991) and system GMM (Arellano and Bover, 1995; Blundell and Bond, 1998) models to address dynamic panel bias and potential endogeneity of regressors.

The empirical analysis sheds light on the relationship between judicial timeliness and economic growth, and it was conducted on a cross-country dataset of 169 countries for a panel of years (2004–2019). Accordingly, it enables us to exploit spatial and temporal variation in JT to explain changes in GDP growth rates. This baseline model includes country-fixed effects as well as a number of standard controls. While the use of country-fixed effects does not solve endogeneity concerns decisively, it essentially tackles the issue of omitted variable bias, *i.e.*, bias arising from unobservable common-cause confounders. By including both lagged growth rates and lagged JT variables, we are able to partially address the possible endogeneity of both growth and timeliness in our dynamic panel specification (Acemoglu et al., 2019). The baseline model we estimate is represented by the following equation with endogenous JT in the presence of unobserved country- and time-fixed effects:

$$\Delta y_{i,t} = \alpha_0 + \sum_{k=1}^{K} \hat{\alpha}_k \cdot \Delta y_{i,t-k} + \hat{\beta} \cdot JT_{i,t} + \sum_{k=1}^{K} \hat{\beta}_k \cdot JT_{i,t-k} + \mathbf{X}'_{i,t}\hat{\pi} + \delta_i + \epsilon_{i,t} \tag{1}$$

where $\Delta y_{i,t}$ represents GDP growth rate between $t-1$ and t in country i at time t, the coefficient vector $\alpha_k = (\alpha_1, \dots, \alpha_K)$ represents lagged state dependence and indicates a source of persistence. Our key parameter of interest is $\hat{\beta}$, which represents the contribution of judicial timeliness variable (JT) to economic growth, where we expect $\hat{\beta} < 0$. The coefficient vector $\beta_k = (\beta_1, \dots, \beta_K)$ captures the past realization of judicial timeliness.

The vector $\mathbf{X}$ comprises a set of structural growth confounders and includes investment share of GDP, government size, trade openness, exchange rate distortions, population growth and population size. Furthermore, δ_i is a full set of country-fixed effects unobserved by the econometrician and comprises time-invariant sources of heterogeneity bias. The stochastic disturbances are denoted by $\epsilon_{i,t}$.

[3] Concerning the third potential source of endogeneity, measurement error, refer to Section 3.1 for a discussion about the robustness of our metric of Judicial Timeliness.

4.2. Panel vector-autoregressive growth model

Before setting out the formal notation, it is important to clarify the scope and interpretation of the panel VAR analysis. The specification is intended to describe the joint reduced-form dynamics of GDP growth and judicial timeliness, treating both variables as endogenous and allowing for feedback effects between them. As such, the estimates do not identify structural causal parameters, and the impulse responses derived from the system should be interpreted as descriptive dynamic associations rather than purely causal effects. This distinction addresses potential confusion in the earlier version of the manuscript, where the notation and discussion could have been read as treating judicial timeliness as exogenous or as implying structural identification.

We also take care to apply the PVAR framework in a way that is consistent with established practice in the empirical macroeconometric literature. Both variables are included on the left-hand side of the system, and lags of each appear in all equations. Estimation is carried out using system GMM, with endogenous regressors instrumented by their own lagged levels and differences, and the instrument set is collapsed to avoid overfitting. Standard specification diagnostics are reported to assess the absence of serial correlation in the residuals, and stability of the estimated system.

Next in order to solve endogeneity issue we estimate the following panel vector-autoregressive (PVAR) semi-differenced linear equations:

$$\Delta y_{i,t} = \sum_{t=1}^{p} b_p \cdot \Delta y_{i,t-p} + \sum_{t=1}^{p} \beta_{JT} \cdot JT_{i,t-p} + \sum_{t=1}^{p} b_X X_{i,t-p} + \varepsilon_{i,t} \tag{2}$$

where Δy is a $1 \times k$ vector of growth rates, JT represents the judicial timeliness variable, X is an additional growth-confounding variable added iteratively to the model, and ε captures transitory shocks and growth impulses. The $k \times k$ vectors $\mathbf{b}$ are the parameters to be estimated. In this specification, k denotes the number of endogenous variables, here $k = 2$ (GDP growth and judicial timeliness). Both variables appear in the vector y_{it} on the left-hand side of the system, and lags of both variables enter all equations on the right-hand side. This ensures that judicial timeliness is treated as fully endogenous, in contrast to the earlier version of the manuscript where the notation could have been read as implying exogeneity.

We estimate the structural relationship between growth and judicial timeliness using fixed-effects transformation to ensure that the effects are not confounded by unobserved heterogeneity. The traditional approach to estimate structural PVAR specification is to use the set of lagged dependent variables. Specifically, the PVAR model is estimated using system GMM as proposed by Holtz-Eakin et al. (1988) and further developed by Love and Zicchino (2006). To address the potential endogeneity of all variables in the system, we treat them as simultaneously endogenous This approach ensures consistent estimation in the presence of feedback effects and lagged dynamics.

Our key assumption is that the transitory shocks to growth and judicial timeliness are serially uncorrelated on average. This ensures that first-difference transformation of Eq. (2) may be consistently estimated by instrumenting the lagged differences with differences and levels of $y_{i,t}$ from earlier periods. Since contemporaneous JT and its lagged level exhibit a high country-level temporal dependence and persistence, the proposed strategy should satisfy the relevance assumption and facilitate a less cumbersome isolation of the relationship between growth and judicial timeliness from both observable and unobservable confounders.

As a remedy, we follow Arellano and Bover (1995) who propose forward orthogonal deviation as an alternative transformation which does not have the weaknesses inherent in FD transformation. Instead of using deviations from past realizations, we subtract the average of all available future observations to minimize the data loss. Hence, since past realizations are not included in this transformation, the instruments remain valid and with $T \geq 4$, realizations are necessary to produce instruments in levels.

It should be noted that in the presence of the contextual panel setting with a large number of countries and relatively short time period, dynamic panel estimators and panel vector autoregression approach are particularly well suited for capturing the dynamic feedback between judicial timeliness and economic growth. Both empirical strategies account for the inherent persistence in growth and judicial timeliness by including lagged variables which is essentially for modeling feedback loop between judicial performance and economic growth. Given per se limited depth and length of our panel, more sophisticated techniques such as structural vector autoregressive models or perhaps even long-run co-integration model would be far less reliable and even infeasible for the purpose of our investigation. By contrast, dynamic panel analysis and panel vector autoregressive technique provide robust estimates in lieu of the intrinsic constraints, ensuring that meaningful insights are not lost despite data limitations.

Additionally, dynamic panel-level estimators and panel vector autoregressive approaches help mitigate the standard endogeneity concerns by leveraging internal instruments and treating both economic growth and judicial timeliness as potentially endogenous which is crucial when judicial performance and economic outcomes such as growth mutually influence each other. Therefore, our choice is a pragmatic one and satisfies a proper balance between the empirical rigor and practical constraints imposed by our available data.

5. Results

5.1. Baseline fixed-effects estimates

Panel A of Table 2 reports the fixed-effects estimated relationship between judicial timeliness and economic growth using output-side growth rates of GDP as the dependent variable. The set of estimated specifications is varied and entails both the full sample of 169 countries as well as distinctive institutional variants (such as civil-law and common-law subsamples), dividing countries according to their level of human capital and growth rate. The re-analysis of the relationship across subsamples is particularly

Table 2
Fixed-effects and dynamic panel estimates, output-side GDP.

	Low growth	High growth	Low HCI	High HCI	Civil-law only	Common-law only	Full sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: fixed-effects estimates							
JT	1.60e−05	−0.000203*	−0.000173*	−2.66e−05	−0.000156*	−6.75e−05	−0.000117**
	(7.42e−05)	(0.000103)	(8.78e−05)	(6.24e−05)	(8.08e−05)	(6.69e−05)	(5.71e−05)
Within R2	0.353	0.114	0.112	0.181	0.157	0.113	0.139
Countries	81	84	77	77	116	49	165
obs	1134	1176	1078	1078	1624	686	2310
Panel B: dynamic panel estimates (levels of treatment variable)							
JT	−6.42e−05	−0.00116***	−0.000520*	−0.000223	−0.000895***	−0.000679*	−0.00112***
	(0.000311)	(0.000424)	(0.000271)	(0.000222)	(0.000235)	(0.000352)	(0.000262)
Countries	81	84	77	77	116	49	165
obs.	972	1008	924	924	1392	588	1980
Panel C: dynamic panel estimates (first differences of treatment variable)							
*d*JT	−0.000122	−0.00115***	−0.000569**	−0.000212	−0.000763***	−0.000485*	−0.00115***
	(0.000306)	(0.000386)	(0.000238)	(0.000194)	(0.000259)	(0.000260)	(0.000310)
Countries	81	84	77	77	116	49	165
obs.	972	1008	924	924	1392	588	1980
Panel D: dynamic panel estimates (endogenous macroeconomic covariates)							
JT	−1.12e−07	−0.000393**	−0.000239**	−2.13e−05	−0.000489***	−7.80e−05	−0.000423***
	(9.98e−05)	(0.000177)	(9.70e−05)	(8.16e−05)	(0.000164)	(0.000106)	(0.000149)
Countries	81	84	77	77	116	49	165
obs	1053	1092	1001	1001	1508	637	2145

Notes: Panel A reports fixed-effects estimated relationship between judicial timeliness and economic growth in a sample of 169 countries for the period 2004–2019. Cluster-robust standard errors at the country level are denoted in the parentheses. Panel B, C and D report (Arellano and Bond, 1991) linear dynamic panel estimates of the same relationship. The dependent variable is output-side growth rate of GDP. All regressions includes country fixed effects and controls for investment share of GDP, government size, trade openness, exchange rate distortions, population growth and size. Panel D treats investment share of GDP, government size, trade openness, exchange rate distortions as endogenous regressors in our GMM specification and instrument them using internal instruments (lagged levels and differences). Standard errors are adjusted for arbitrary heteroskedasticity and serially correlated stochastic disturbances within countries into country-specific and temporal clusters using non-nested multi-way clustering scheme for finite-sample adjustment of the empirical distribution function. Asterisks denote statistically significant coefficients at 10% (*), 5% (**), and 1% (***), respectively.

informative, as it allows us to gauge the variance of our relationship of interest for countries with different degrees of court-dependent business environments. Each specification contains the full set of country and year fixed effects and the full set of structural control variables.

Column (7) of Panel A reports the relationship between judicial timeliness and economic growth estimated over the full sample. The estimated coefficient on judicial timeliness is positive and statistically significant at 5%. The estimated coefficients on the first and second lag of JT do not appear to be statistically significant, which implies that anticipatory effects of increasing delays are seldom noticeable and only weakly perceivable. Without loss of generality, full-sample estimates suggest that lengthier judicial procedures are quite strongly negatively associated with the GDP growth rate. In order to properly interpret the magnitude of our estimated coefficients it is worth noticing that our dependent variable (GDP growth rates) is expressed in percentage point, while our regressor of interest (Judicial Timeliness) is measured in days. Our point estimate (−0.000117) from column (7) suggests that one standard deviation increase in JT (303.01 days or about 10 months of disposition time) is associated with a reduction of 3.5 percentage points of the yearly GDP growth rate ($-0.000117 * 303.01 = -0,035451$ or slightly more than 3.5%). We also tried to exploit 5 years averages instead of year-to-year changes in JT. Despite minimal changes in the model due to a sharp reduction in the number of observations, estimates (not shown in the table but available upon request) remain similar both in size and significance. In Columns (1) through (6) we investigate whether judicial timeliness has a more pronounced relationship with economic performance in court-dependent business environments.

In Columns (1) and (2) we distinguish countries according to their average growth rate being, respectively, below or above median. It emerges that the overall coefficient estimated in column (7) derives from economies with above-median growth trajectories. The estimated coefficient for high growing countries is 73% higher than the full sample's one, while the one for slowly growing economies is not statistically different from zero.

In Columns (3) and (4) we follow Bosio et al. (2022) and divide countries into two sub-samples distinguishing between high/low (above/below median) Public Sector Capacity (PSC) in order to account for differences in levels of human capital. We observe that the significance of the relationship of interest seems to be driven by countries with low PSC, while judicial timeliness appears not related to growth in high PSC countries. Countries with low human capital appear to be affected about 48% more by the relationship of interest than the average country.

Column (5) presents the baseline estimate restricted to the subsample of civil-law countries, confirming our baseline evidence of negative but still statistically significant coefficient of the judicial timeliness variable. In particular, it emerges that the relationship

between judicial timeliness and economic growth is 33% stronger in civil law countries than in the full sample. By contrast, column (6) considers common-law countries and indicates no relationship between judicial timeliness and economic growth. Therefore, our evidence suggests that the estimated relationship is prevalent in civil-law jurisdictions.

5.2. Dynamic panel estimates

Panels B and C of Table 2 report (Arellano and Bond, 1991) dynamic panel estimates of the relationship between judicial timeliness and GDP growth. In order to account not only for the levels (Panel B) of judicial timeliness, but also its changes, we have re-estimated Eq. (1) using first differences of JT[4] (Panel C).

The evidence consistently reinforces the estimated association between judicial timeliness and GDP growth. Our coefficient of interest gains substantial strength in magnitude and statistical significance. In column (7) of Panel A our full sample estimates are now significant at the 1% level, while the coefficient's size increases almost tenfold: a 100 days decrease in JT (about one third of a standard deviation) is now associated with an 11% boost to economic growth. In columns (1) through (6) we pursue our distinction between countries according to their degree of court-dependent business environments, finding consistent results. The association between judicial timeliness and GDP growth remains around 30% stronger in civil law countries than in common law ones. The same appears to be tenfold stronger in high growth countries than in their low growth counterparts. Economies with low human capital are more heavily hit by judicial timeliness than countries with higher human capital. Adopting first differences of JT rather than its levels as done in Panel B, does not alter our results.

5.3. Dynamic panel estimates under endogenous macroeconomic controls

One potential concerns behind the dynamic panel estimates is that some of our macroeconomic control variables may themselves be influenced by judicial timeliness, thus raising the possibility of endogeneity bias. We re-estimate our dynamic panel specification allowing for endogeneity in key structural growth controls. In our baseline dynamic model, these controls were included in levels without instrumentation, which implicitly assumed they were strictly exogenous. While this assumption is common in empirical growth regressions, it is not innocuous in the present context. For example, both the investment share of GDP and government size could plausibly respond to institutional quality, including courts' performance, through changes in investor confidence, fiscal policy, and public sector resource allocation. Similarly, trade openness and exchange rate distortions may be affected by the legal environment through their influence on transaction costs, trade policy credibility, and exchange rate stability.

In light of this, we reconfigure the baseline dynamic panel specification to explicitly treat investment share of GDP, government size, exchange rate distortions, and trade openness as endogenous regressors. By contrast, the demographic variables, namely, population size and population growth, are retained as exogenous regressors on theoretical grounds, as they are unlikely to be directly affected by short-run changes in judicial timeliness within our sample period. The results, presented in Panel D of Table 2, confirm the robustness of our main findings. In the full-sample specification (column 7), the coefficient on judicial timeliness remains negative and statistically significant at the 1% level. The same holds also for our disaggregated sub-samples.

Overall, this particular analysis directly addresses the concern that our results might be driven by mechanical correlations between judicial timeliness and other macroeconomic variables. By treating the relevant controls as endogenous and instrumenting them, we mitigate potential bias arising from such channels. The fact that the results remain essentially unchanged, both in sign and magnitude, strengthens our confidence that the observed negative relationship between judicial delays and economic growth is a robust empirical regularity rather than a modeling artifact.

5.4. Decomposing by level of per capita GDP

Perhaps the most important question arising from our results concerns the heterogeneity of such relationship with respect to the level of per capita GDP. Such an analysis may partially unravel whether richer or poorer countries are more disproportionately hurt by increasing judicial slowness in terms of the growth shortfall and subsequent slowdown.

Using the World Bank's four-fold classification of countries into income groups[5] we estimate the baseline growth specification by piecewise excluding each income block from the full-sample specification to tackle the sensitivity of the baseline estimate to the exclusion of group blocks, estimates are reported in Table 3. When excluding high income countries as in column (1), the estimated coefficients remains statistically significant at 5% and confirm the negative coefficient of JT. In columns (2) and (3) we exclude, respectively, lower-middle and upper-middle income countries from the sample. We confirm a positive relationship between judicial timeliness and economic growth which however turns out to be not statistically significant. In column (4)we then exclude high-income economies from the full sample we obtain a negative and statistically significant coefficient is confirmed at 1% significance threshold.

Taken altogether, the empirical decomposition of the relationship between judicial timeliness and economic development by income groups suggests that increases in judicial delay hurt middle-income economies more than both low and high-income economies.

[4] We would like to thank Matthieu Chemin for this suggestion.

[5] Low income, lower-middle income, upper-middle income, high income.

Table 3
Fixed effects estimates by income groups.

	w/o low income countries	w/o lower-middle income countries	w/o upper-middle income countries	w/o high income countries
	(1)	(2)	(3)	(4)
JT	−0.000120**	−5.63e−05	−9.60e−05	−0.000219***
	(6.09e−05)	(6.23e−05)	(6.33e−05)	(7.61e−05)
R2	0.162	0.198	0.097	0.133
Countries	141	125	117	112
obs	1974	1750	1638	1568

Notes: the table reports the estimated relationship between judicial timeliness on the growth rate of GDP using income group-restricted estimates in columns (1) through (4) by making use of World Bank's four-fold classification of countries into income per capita groups. Standard errors are adjusted for serially correlated stochastic disturbances and arbitrary heteroskedastic distribution of random error variance using finite-sample adjustment of the empirical distribution function and cluster-robust coefficient inference to remove the inconsistencies arising from biased OLS variance–covariance matrix estimator. Country cluster-robust standard errors are denoted in the parentheses. Asterisks denote statistically significant sample regression coefficients at 10% (*), 5% (**), and 1% (***), respectively.

Table 4
Quantile regressions estimates.

	0.05	0.10	0.25	0.50	0.75	0.90	0.95
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
JT	4.34e−06	3.73e−06	−2.13e−06	−5.13e−06	−.00001**	−.00002***	−.00002*
	(1.07e−05)	(1.10e−05)	(4.95e−06)	(3.86e−06)	(5.79e−06)	(7.97e−06)	(.00001)
R2	0.037	0.038	0.056	0.011	0.016	0.015	0.019
obs	2475	2475	2475	2475	2475	2475	2475

Notes: the table reports the relationship between judicial delays and GDP growth rate in a sample of 169 countries for the period 2004–2019 by making use of Koenker (2005) quantile regression panel-level estimator. The robust variance–covariance matrix is computed and allows for serially correlated disturbances, potentially heteroskedastic distribution of error variance, and intra-cluster correlation using (Machado and Santos Silva, 2019) quantile heteroskedasticity test. The standard errors are asymptotically valid, robust to model misspecification, and robust to between-country and intra-country serially correlated residuals using (Parente and Santos Silva, 2016) intra-clustering scheme to adjust the empirical distribution function using the finite-sample properties. Standard errors are reported in the parentheses. Asterisks denote statistically significant quantile-specific regression coefficients at 10%(*), 5% (**), and 1% (***), respectively.

5.5. Decomposing by growth quantiles

Estimations in Table 2 already show that the negative relationship between economic growth and judicial timeliness tends to be stronger and more statistically significant for countries with an above-median economic growth. The next question to ask concerns a further decomposition of this relationship by growth quantiles. To address this point, we performed quantile-specific estimates of the growth-timeliness relationship across various tails of the growth distribution estimated using quantile regression estimators and confidence intervals and null hypothesis test to conduce inference. This particular approach allows us to examine whether changes in judicial delays tend to hurt the economies when they enter the growth phase of the business cycle and, at least in principle, improve their capability in judicial dispute resolution. We distinguish between six different tails of the growth distribution, ranging from the 5th percentile to the 95th percentile.

Table 4 reports our quantile analysis results. The estimated quantile-specific JT coefficient is statistically insignificant from the 5th percentile to the median. The estimated relationship between growth and courts' delays is particularly strong from the 75th percentile to the 95th percentile. This implies that high-growth economies undergoing a more rigorous growth acceleration appear to be most severely affected by prolonged judicial time. Since the lagged values of the growth variables are properly included in the growth regressions, it is unlikely that past persistence or idiosyncratic shocks would be the key driver of the difficulty of transition back to high-growth equilibrium.

5.6. Bias corrected estimates

To address potential concerns regarding dynamic panel bias, commonly referred to as Nickell bias, we implement the bias-corrected fixed effects estimator developed by Breitung et al. (2022). Table 5 reports estimates of the dynamic relationship between judicial timeliness and GDP growth obtained using this estimator, which analytically corrects for the small-T bias in fixed effects models with lagged dependent variables. The results are presented for the same samples analyzed in Sections 5.1 and 5.2, thereby enabling a direct comparison with our baseline fixed-effects and dynamic panel specifications.

The evidence confirms a negative and statistically significant association between delays in judicial proceedings and the rate of GDP growth. According to our preferred full-sample specification in column (7), a one standard deviation deterioration in judicial timeliness is associated with a 0.3 percentage point reduction in the annual growth rate of GDP, significant at the 5% level. These findings are robust and substantively consistent across alternative sub-samples, particularly within civil law jurisdictions, low-human capital environments, and fast-growing economies, where the detrimental effects of judicial delay appear most pronounced.

Notably, the magnitude of the coefficients in the bias-corrected estimates closely aligns with those obtained in our baseline analyses. This correspondence suggests that any downward distortion due to Nickell bias, while theoretically relevant in panels with

Table 5
Bias-corrected fixed effects estimates.

	Low growth	High growth	Low HCI	High HCI	Civil-law only	Common-law only	Full sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
JT	−.00004	−.0001*	−.0002**	.00002	−.0001*	−.00001	−.0001**
	(.00007)	(.0001)	(.00008)	(.00005)	(.00006)	(.00009)	(.00005)
Nickel bias correction	YES	YES	YES	YES	YES	YES	YES
Countries	81	83	72	77	142	22	164
obs.	1134	1153	999	1078	1979	308	2287

Notes: the table reports bias-corrected (Breitung et al., 2022) fixed-effects dynamic panel estimates correcting the first-order condition of the classical fixed-effects estimator through a set of nonlinear moment conditions that can be solved with conventional Gauss–Newton numerical methods. The dependent variable is output-side growth rate of GDP. All regressions include country fixed effects and controls for investment share of GDP, government size, trade openness, exchange rate distortions, population growth and size. Standard errors are adjusted for arbitrary heteroskedasticity and serially correlated stochastic disturbances within countries into country-specific and temporal clusters using non-nested multi-way clustering scheme for finite-sample adjustment of the empirical distribution function. Asterisks denote statistically significant coefficients at 10% (*), 5% (**), and 1% (***), respectively.

Table 6
Intermediate outcomes.

Variables	TFP	labor % GDP	Capital stock/L	IRR	Residual trade % GDP
	(1)	(2)	(3)	(4)	(5)
JT	−7.20e−05*	4.62e−05	−3.42e−05*	−2.05e−05*	3.59e−05
	(4.06e−05)	(3.20e−05)	(1.82e−05)	(1.18e−05)	(2.70e−05)
R2	0.768	0.582	0.973	0.471	0.339
Countries	116	131	166	131	169
obs	1624	1834	2313	1834	2366

Robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

a limited temporal dimension, is unlikely to materially affect our conclusions, particularly given that our time dimension exceeds the conventional thresholds ($T > 10$) below which, such bias is most severe. Overall, the results bolster the empirical credibility of our main findings and further underscore the importance of judicial timeliness for long-run economic performance.

5.7. Intermediate outcomes

Since it is nearly impossible to understand the nexus between judicial delays and growth without a more elaborate analysis of the intermediate outcomes, the postulated channels are important to unravel specific pathways through which increasing delays ultimately dampen growth dynamics. In Table 6, we empirically explore some of the transmission mechanisms at work behind the negative relationship between increasing judicial delays and economic growth.

Previous research on accounting for sources growth invariably suggests that between 50 percent and 70 percent of cross-country per capita income gaps can be explained by the differences in total factor productivity (Hsieh and Klenow, 2010).

To this end, we use (Feenstra et al., 2015) measure of TFP level relative to the United States frontier and link it with the judicial delay variables. By estimating the baseline dynamic panel-level specification for the intermediate outcomes, we show that increasing judicial delays tend to produce statistically significantly lower total factor productivity both across and within countries. The results are reported in greater detail in the supplementary online appendix. By contrast, prolonged judicial delays are associated with a marked and notable reduction in capital stock per worker although the magnitude of the coefficient is about half as large as in the TFP specification. Longer judicial delays are also associated with a notable and statistically significant reduction in overall rate of return on investment although it should be noted that the magnitude of the relationship is around 40 percent lower compared to the stock of capital per worker.

The empirical evidence almost unequivocally suggests that deteriorating court performance tends to have non-trivial consequences for rate of return on the investment and is consistent with prior evidence (Srhoj et al., 2023). We also find some evidence that prolonged judicial delays tend to proliferate the incentives for informality (Galiani and Weinschelbaum, 2012). Increasing time to disposition is associated with a somewhat larger residual share of GDP, suggesting that increasing delays tend to increase the cost of formality, although the estimated coefficient is not statistically significant at conventional levels and pales smaller and less substantial compared to capital stock, return on investment and TFP.

6. Robustness

6.1. Panel vector-autoregressive estimates

Results from Tables 2 suggest a negative and significant relationship between judicial timeliness and growth. In this section we wish to further investigate the chain and arrow of causation between institutional performance and growth (Glaeser et al., 2004).

Table 7
Panel VAR estimated effects of judicial timeliness on economic growth.

Conditioning variable ($Y2$)	Investment GDP share	Government size	Trade openness	Exchange rate	Population growth	Population size
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Vector-autoregressive coefficients set						
$Growth_{(t-1)}$	.014	.041	.038	.076	.081	.078
	(.246)	(.094)	(.108)	(.099)	(.092)	(.086)
$JT_{(t-1)}$	−.006	−.0006**	−.002*	−.003*	−.002*	−.001*
	(.012)	(.0003)	(.001)	(.002)	(.001)	(.0009)
Panel B: Vector-autoregressive coefficients set: judicial delay sub-specification						
$JT_{(t-1)}$	1.270	.990***	1.152***	1.156***	1.189***	.873***
	(1.282)	(.043)	(.302)	(.384)	(.422)	(.150)
$Growth_{(t-1)}$	−.078	−.069	−.095	−.096	−.096	−.078
	(.267)	(.078)	(.122)	(.111)	(.123)	(.071)
Panel C: Wald-Granger causality test p-values						
JT	[0.828]	[0.036]	[0.076]	[0.101]	[0.117]	[0.100]
Y2	[0.799]	[0.001]	[0.086]	[0.905]	[0.001]	[0.283]
Countries	169	169	169	169	169	169
obs.	2197	2197	2197	2197	2197	2197

Notes: the table reports panel vector autoregressive estimates of the relationship between judicial timeliness and GDP growth for a sample of 169 countries in the period 2004–2019 assuming a simultaneous interdependence between growth, judicial delays and the growth confounding variables reported in each column. The standard errors are adjusted for arbitrary heteroskedasticity and serially correlated stochastic disturbances at the country level. Asterisks denote statistically significant sample coefficients at 10% (*), 5% (**) and 1% (***), respectively.

Do longer delays decrease GDP growth? Or conversely, are slow-growing countries less able to afford more efficient judiciary? Panel vector-autoregressive framework allows us to investigate this issue.

Table 7 reports panel vector-autoregressive estimates of the growth specification assuming both growth and judicial timeliness as simultaneously interdependent and endogenous variables. Such an approach allows us to partially address the concerns that might arise from the endogeneity of judicial timeliness, despite the full set of fixed effects and lagged values of both key variables being included in the model. In addition, the direction of causation between judicial timeliness and economic growth can be appropriately tested in the framework of interdependent and simultaneously endogenous variables. By disentangling the relationship, our analysis should be able to uncover whether longer judicial delays induce growth shortfall or, conversely, whether deteriorating economic conditions increase judicial delays.

To avoid the risk of model over-dimensionality, each auxiliary growth confounder from the baseline specification is separately added to the panel VAR model specification (Sala-i Martin et al., 2004). It should be noted that Table 7 reports the results of two specifications. More specifically, Panel A reports the results of the specification where the dependent variable is the GDP growth rate whereas the lagged JT is the key variable of interest. By contrast, Panel B reports the results of the estimated specification where timeliness is the dependent variable whereas the lagged rate of GDP growth is the primary explanatory variable. As in the standard panel VAR setting, two-equation framework allows us to better unravel whether more prolonged judicial procedures precede lower rate of GDP growth or vice versa.

The evidence from panel VAR estimated specifications confirms our prior results. It suggests that increasing judicial delays is both negatively and statistically significant within 5% to 10% bounds in five out of six specifications with GDP growth. The estimated structural parameter on the lagged JT variable is within the range of −0.0006 to −0.003 and appears to be consistent with our baseline estimates. At the minimum, our estimates imply that decreasing judicial delays by 100 days is associated with an increase in GDP growth rate of 0.06 percentage points, *ceteris paribus*. Panel C reports Wald-Granger causality tests between the full set of interdependent variables, and indeed suggests that the null hypothesis of no structural relationship between timeliness and growth can be summarily rejected, whilst the evidence against the null hypothesis between growth confounders and growth rate is more uncertain. Therefore, our analysis offers relatively strong support for a chain of causation that runs from increased judicial delays to lower GDP growth.

It should be noted that interpreting coefficients from individual dynamic panel equations can be misleading when estimating the causal impact of institutional shocks, especially in the presence of endogeneity and dynamic feedback. Therefore, we estimate impulse response functions (IRF) derived from our baseline PVAR framework, which allow us to trace the evolution of GDP growth following a structural shock in judicial timeliness variable.

Fig. 2 presents the IRF of the output-side GDP growth rate to a one-standard-deviation shock in judicial timeliness, which roughly corresponds to a 10 months increase in disposition time. The PVAR is estimated with country fixed effects, using a GMM-based estimator that accounts for endogeneity and potential Nickell bias. Following a standard recursive identification strategy using Cholesky decomposition, our strategy is to focus on judicial timeliness as the underlying impulse variable under the plausible assumption that judicial conditions can affect GDP growth contemporaneously, while the overall output responds to these frictions only with a lag.

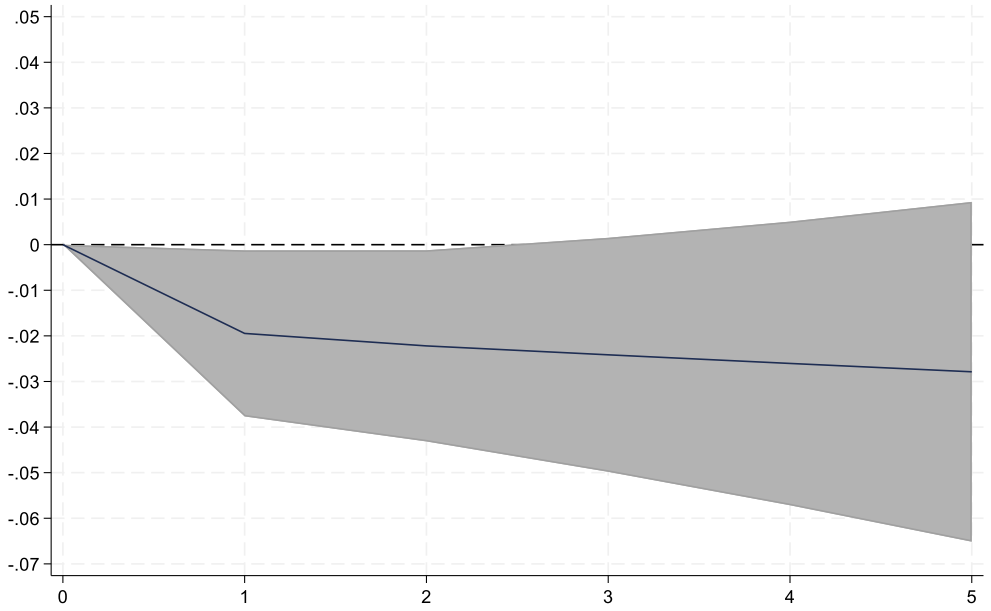

Fig. 2. Dynamic effects of judicial delay: panel VAR impulse response analysis.

The impulse response reveals that a deterioration in judicial timeliness results in a significant decline in GDP growth in the initial periods, with the maximum contraction occurring within the first year following the shock. Specifically, the growth response drops by approximately 0.02 to 0.03 percentage points, with the 95% confidence bands excluding zero. In subsequent periods (years 2 to 5), the negative impule persists, albeit with gradually widening confidence intervals, as expected in macro-panel settings. Most importantly, the point estimate remains negative throughout the five-year horizon, providing evidence of persistent macroeconomic costs associated with institutional inefficiency arising from the deteriorating judicial timeliness.

Our dynamic analysis based on the IRF complements and reinforces the findings from our fixed-effects and bias-corrected panel regressions. The similarity in magnitude and direction of the impact, a reduction in growth of roughly 0.3 percentage points for a one-standard-deviation increase in delay, testifies to the robustness of our results. Moreover, the IRF results demonstrate that the economic consequences of deteriorating judicial quality are not merely short-lived or static but propagate over time in a manner consistent with institutional theories of long-run development and contract enforcement.

6.2. Randomization inference

To address the robustness of our main findings and directly respond to concerns about the possibility of spurious effect, we implement a two-step randomization inference strategy. This approach tests whether the estimated coefficient of judicial delays on economic growth could plausibly emerge under arbitrary reassignment of the treatment variable within the panel structure. In the first step, we randomly assign values of the judicial timeliness variable within each country block across both the full sample and the subsamples used in Sections 5.1 and 5.2. This is done by drawing a uniformly distributed random value over the observed empirical support of judicial timeliness within the panel. In the second step, we extend this procedure by implementing two alternative randomization schemes. First, we reshuffle values around the lower bound of the judicial timeliness variable, generating placebo distributions with systematically low delay values. Second, we repeat the procedure around the upper bound, producing placebo values concentrated at the higher end of the distribution. These reshuffling variants serve to test whether the stability of our estimates is sensitive to the support of the assigned variable, and whether extreme permutations could falsely generate significant placebo effects.

For each reshuffled variant, we re-estimate the baseline fixed-effects growth from Section 5.1 and focus on the resulting placebo coefficient, denoted $\hat{\beta}_{j,t}^{Placebo}$. The central testable implication is whether the coefficient from the baseline specification is statistically distinguishable from its permutation-based counterpart. In particular, we evaluate the null hypothesis $H_0 : \hat{\beta}_{j,t}^{FE} = \hat{\beta}_{j,t}^{Placebo}$ where the failure to reject H_0 indicates that the estimated coefficient is non-spurious and robust to within-block reassignment. Conversely, if the significance of placebo estimate vanishes across random permutations, this strengthens the inference that our main result is not easily replicable by chance under arbitrary permutations. Importantly, we bootstrap standard errors more than 10,000 replications to compute the permutation p-values and construct the 95% confidence intervals. This variant of the high-replication design ensures a tight empirical distribution for the test statistics and mitigates concerns about the excessive and potentially unstable sampling variable within the simulated placebo effects.

Table 8
Placebo test of judicial timeliness and economic growth, 2004–2019.

	Within-block randomization strata						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Randomization block	Low Growth	High Growth	Low HCI	High HCI	Civil Law only	Common Law only	Full sample
Panel A: Full bound within-country block reshuffling of JT variable							
$\hat{\beta}_{j,t}^{Placebo}$	−.00001	.000002	−.0000846	.000018	−.000074	−.000004	−.00005
	(.00005)	(.000053)	(.0000594)	(.000042)	(.000043)	(.000007)	(.00004)
$\approx$ p-value on $H_0 : \hat{\beta}_{j,t}^{FE} = \hat{\beta}_{j,t}^{Placebo}$	[0.081]	[0.961]	[0.164]	[0.676]	[0.133]	[0.542]	[0.174]
Panel B: Minimum bound within-country block reshuffling of JT variable							
$\hat{\beta}_{j,t}^{Placebo}$	−.00001	−.000008	−.000012	.000092	−.00001	.0000052	−0.000009
	(.00002)	(.000025)	(.000029)	(.000028)	(.00002)	(.0000312)	(.00001)
$\approx$ p-value on $H_0 : \hat{\beta}_{j,t}^{FE} = \hat{\beta}_{j,t}^{Placebo}$	[0.532]	[0.973]	[0.674]	[0.745]	[0.492]	[0.869]	[0.618]
Panel C: Maximum bound within-country block reshuffling of JT variable							
$\hat{\beta}_{j,t}^{Placebo}$	−.00001	.000001	−.000462	.000021	−.0000023	.00002	−.0000039
	(.00002)	(.000002)	(.000027)	(.000301)	(.000021)	(.00002)	(.00001)
$\approx$ p-value on $H_0 : \hat{\beta}_{j,t}^{FE} = \hat{\beta}_{j,t}^{Placebo}$	[0.532]	[0.556]	[0.987]	[0.944]	[0.911]	[0.372]	[0.838]
# bootstrap replications	10,000	10,000	10,000	10,000	10,000	10,000	10,000

Notes: This table reports the results of placebo tests based on within-country block randomization of the judicial timeliness (JT) variable. Each panel presents estimates from reshuffling the JT variable across countries, preserving the panel structure while severing the empirical link between judicial efficiency and economic growth. The dependent variable in all specifications is the GDP growth rate. Each column corresponds to a distinct subsample used in the main analysis. Panel A shows results based on full-bound reshuffling of the JT variable within each country block, drawing values uniformly from the entire support of the variable. Panels B and C impose restrictions on the permutation bounds, limiting randomization to the lower and upper bounds of the observed JT distribution, respectively. This permutation design tests whether spurious effects are concentrated near particular regions of the distribution. For each permutation setup, the placebo coefficient and its bootstrapped standard error are reported, followed by the p-value testing the null hypothesis that the placebo coefficient equals the baseline fixed-effects coefficient from the main specification The resulting P-values (reported in square brackets) are computed over 10,000 bootstrap replications using clustered standard errors at the country level (reported in parentheses).

Table 8 presents the results of our first-stage randomization inference procedure, designed to test whether the observed relation of judicial timeliness and GDP growth could arise spuriously through arbitrary reassignments of the treatment variable. The analysis is based on 10,000 bootstrap replications, and the placebo coefficients $\hat{\beta}_{j,t}^{Placebo}$ are reported across a range of within-country block randomization strategies. Panel A implements a full-bound reshuffling procedure, where the values of the judicial timeliness (JT) variable are randomly reassigned within each country using the full support of the observed variable. In the full-sample model estimates in column (7) and across subsamples, the resulting placebo coefficients are statistically indistinguishable from zero. Approximate p-values on the null hypothesis $H_0 : \hat{\beta}_{j,t}^{FE} = \hat{\beta}_{j,t}^{Placebo}$ exceed standard significance thresholds in every specification. Notably, the full-sample p-value of 0.174, reported in column 7, reinforces the robustness of our main results. Even when values are reshuffled uniformly across the observed support, placebo effects do not replicate the strength or statistical significance of the baseline fixed-effects estimate.

Furthermore, Panels B and C assess whether the results are sensitive to the position of the support used for permutation. In Panel B, the reshuffling is constrained to the minimum bound of judicial timeliness, simulating a setting where each block is randomly assigned low-delay values. Similarly, Panel C performs the permutation around the maximum bound, assigning higher-delay values within each block. In both cases, we again fail to reject the null hypothesis across all subsamples. Approximate p-values remain well above conventional thresholds, ranging from 0.372 to 0.973 depending on the subsample. Importantly, even in the full-sample specification, the p-values for Panels B and C are 0.618 and 0.838, respectively, further emphasizing the difficulty of replicating significant placebo effects under arbitrary reassignment of JT within countries.

Taken together, the evidence from all three reshuffling variants strongly suggests that the estimated relationship between court performance and economic growth is not easily recoverable under random permutations. The placebo coefficients are consistently close to zero, and far from the magnitude and significance of the baseline estimates reported in Sections 5.1 and 5.2. These results lend robust support to the interpretation that the main findings are unlikely to be statistical artifacts or the result of overfitting within a short temporal dimension. Rather, the results from Table 8 affirm that the negative relation between growth and judicial delays is plausably both non-spurious and non-replicable by design.

To strengthen the credibility of our interpretation and address the possibility that our baseline findings might be driven by spurious temporal patterns or model-dependent inference, we implement a Monte Carlo simulation based on within-country block randomization. This approach serves as a robust placebo framework and directly confronts the sharp null hypothesis that the observed relationship between judicial delays and economic growth could be replicated under arbitrary timing of treatment exposure. More specifically, we permute the timing of judicial delays within each country block, thereby preserving all relevant cross-sectional heterogeneity, such as institutional quality, legal origin, and macroeconomic conditions, while randomizing the timing structure of judicial inefficiency. This strategy ensures that any systematic relationship detected under the actual data-generating process is not an artifact of spurious intertemporal alignment or unobserved global shocks.

Table 9
Within-block randomization of the relation between judicial timeliness and economic growth, 2004–2019.

		Excluded sub-sample block			
Randomization strata	Full sample	w/o bottom 10%	w/o bottom 25%	w/o top 25%	w/o top 10%
	(1)	(2)	(3)	(4)	(5)
Panel A: Small-Scale Monte Carlo Simulation (# permutation replications = 1000)					
Observed F-statistics	4.423	5.783	5.492	5.546	6.257
Randomization p-value	[0.046]	[0.048]	[0.038]	[0.026]	[0.014]
95% CI on p-value	[0.033, 0.059]	[0.034, 0.061]	[0.026, 0.049]	[0.016, 0.035]	[0.006, 0.021]
Panel B: Large-Scale Monte Carlo Simulation (# permutation replications = 10,000)					
Observed F-statistics	6.675	5.783	5.492	5.545	6.257
Randomization p-value	[0.013]	[0.037]	[0.054]	[0.022]	[0.018]
95% CI on p-value	[0.012, 0.016]	[0.033, 0.041]	[0.049, 0.058]	[0.019, 0.024]	[0.015, 0.021]

Notes: the table reports the results of a within-country block permutation analysis evaluating the robustness of the relationship between judicial delays and GDP growth over the period 2004–2019. Panel A, reports a small-scale Monte Carlo simulation with 1000 permutation replications; Panel B expands the simulation to 10,000 replications to increase statistical power. Each column reports results from a different sample configuration: the full sample (column 1), and subsamples that sequentially exclude the bottom 10%, bottom 25%, top 25%, and top 10% of countries based on average growth rates (columns 2 through 5). For each configuration, we report the observed F-statistic from the fixed-effects growth regression using the true judicial delay variable, followed by the corresponding randomization-based *p*-value and its empirical 95% confidence interval estimated from the permutation distribution. The permutation test evaluates the null hypothesis that the observed F-statistic could have arisen under random treatment timing within country blocks. The rejection of the null supports the claim that the estimated coefficient is unlikely to emerge under arbitrary timing structures and is not easily replicable by chance. Randomization-based p-values are reported in square brackets.

This procedure offers two distinct advantages over conventional inference. First, by design, it disentangles true treatment effects from mechanical associations with unobserved time-varying confounders, especially those correlated across countries but not explicitly modeled. Second, the randomization inference framework is non-parametric and exact under the null, making it particularly well-suited to moderate-T panels where conventional asymptotic approximations may be unreliable. By leveraging Monte Carlo replications of permuted datasets, we generate an empirical distribution of placebo F-statistics, against which the observed statistic can be compared to assess statistical significance in a way that is both transparent and assumption-light.

Table 9 presents the results of our Monte Carlo permutation simulations designed to assess the robustness and credibility of the estimated relationship between judicial delays and economic growth. Column (1) reports results for the full panel, while columns (2) through (5) sequentially exclude potentially high-leverage observations, including the bottom 10%, top 10%, and the lowest and highest quartiles of countries based on average growth rates. Such multi-pronged approach allows us to systematically evaluate whether the significance of our main coefficient is robust to sample perturbations and outlier adjustments. Our focal quantity of interest is the observed F-statistic on the judicial timeliness variable from the baseline growth regression. The key null hypothesis posits that the observed F-statistic is statistically indistinguishable from what would be expected under a regime of random treatment timing, i.e., if judicial delays were assigned arbitrarily within country blocks. Rejection of this null therefore implies that the observed association between judicial timeliness and GDP growth is not easily replicated by placebo treatments, and that the coefficient we estimate reflects a substantive underlying process rather than noise.

Panel A presents the results of a small-scale Monte Carlo simulation using 1000 random permutations of the treatment variable within country blocks. In our preferred full-sample specification (column 1), the null hypothesis is rejected at the 5% significance level, with a randomization *p*-value of 0.046 and a confidence interval that tightly 95% confidence bounds of the *p*-value distribution [0.033, 0.059]. Notably, this finding holds consistently across all subsample variants: the p-values remain below 0.05 or very close to it, even when we exclude either extreme deciles or quartiles of the growth distribution. To increase statistical power and minimize type II error, Panel B extends this analysis to a large-scale Monte Carlo simulation with 10,000 replications. The results here are even more decisive: the randomization *p*-value for the full sample drops to 0.013, with a sharply bounded empirical confidence interval of [0.012, 0.016]. Importantly, this result is not unique to the full sample. Across all exclusion restrictions, the observed F-statistics consistently fall in the upper tail of the permutation distribution, and all associated p-values lie below conventional significance thresholds. The empirical 95% confidence intervals across columns (2) through (5) confirm that these results are not driven by a small number of outlying country-specific observations, and are not sensitive to full sample trimming.

Taken together, this evidence provides further statistical validation of our main findings. The fact that the observed test statistics are not easily replicable under random permutations, across both small- and large-scale simulation regimes, indicates that our results are unlikely to be spurious or data-driven artifacts. The combination of within-block permutation and high-replication Monte Carlo simulation represents a conservative and rigorous inference strategy that strengthens the internal validity of our estimates. This reinforces our core conclusion: judicial delays relates robustly and meaningfully to GDP growth and such relation cannot be dismissed as a mere statistical coincidence.

7. Conclusions

We empirically assess the existence of a positive relation between judicial timeliness and economic growth in court-dependent business environments. Our sample comprised 169 countries for the 2004–2019 period for which we used several indicators of judicial timeliness and examine their contribution to the trajectories of economic growth.

Our results suggest that court's poor performances can be considered as detrimental to economic growth. Our results are closest in spirit to Djankov et al. (2003a), who hypothesize that in certain countries effective institutions shift out the production possibility frontier of the economy. We show evidence that courts, as public institutions, indeed matter in countries where judicial enforcement is more relevant in resolving commercial disputes. High growing economies are characterized by impersonal transactions more depending on courts than the type of relational contracting typical of low growth countries. As a consequence, economic growth is 73% more influenced by judicial enforcement. The relationship is also 47% more constraining for economies characterized by low human capital, in which alternative dispute resolution mechanisms are less frequently available. Civil-law countries and their more complex procedures are 33% more adversely affected by increasing judicial delay.

Our estimated coefficients for JT are robust to a variety of specification checks across a multitude of static and dynamic panel-estimators. The relation between judicial timeliness and economic growth is particularly strong in those business environments more relying on judicial enforcement as high growing economies, civil law jurisdictions and low human capital countries. By treating economic growth, its confounders and judicial timeliness as simultaneous endogenous variables, our evidence confirms – despite the inherent limitations – the strong relationship between judicial timeliness and GDP growth, and rules out with strong confidence the hypothesis that deteriorating economic conditions lead to prolonged judicial delays.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: Simeon Djankov reports financial support was provided by John Templeton Foundation. 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.jebo.2025.107235.

Data availability

Data will be made available on request.

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