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# *Is justice delayed justice denied?*

## An empirical approach<sup>\*</sup>

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### Abstract

Improving judicial performance in order to enhance the business environment has been a policy goal for many governments in the last decades. Following the suggestions of several international organizations, most countries have tried to speed up their case resolution systems by streamlining judicial procedure. However, not as much attention has been devoted to test the potential drawbacks of similar reforms in terms of supplying a quicker but yet qualitatively inferior justice, thus contradicting the well-known legal maxim *justice delayed is justice denied*. The present work wishes to contribute to the empirical literature on the topic by proposing two alternative ways to further disentangle the relationship between judicial performance and judicial quality. Exploiting a dataset of 171 countries for the 2003-2016 time period, we find statistically significant evidence of a strong and negative relationship between courts' delay and countries' quality of the justice. While the intrinsic limits of this kind of institutional empirical analysis suggest caution when interpreting our estimates as proof of causality, we present more robust evidence suggesting that countries characterized by faster judiciaries seem to be equally not affected by a deterioration of the quality of justice, thus confirming the aforementioned maxim, at least descriptively.

**Keywords:** Judicial Delay, Judicial Quality, Empirical Institutional Analysis

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### 1. Introduction

In the last two decades, the legal maxim, *justice delayed is justice denied* has been at the center of domestic and international policies agendas all around the world, especially for its economic consequences. If economic theory has always assumed property rights to be smoothly enforced by courts (Botero et al., 2003; North, 2005), nowadays it

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is quite obvious that even an efficient rule will at best operate ineffectively if not properly enforced (Marciano et al., 2019). Given the incomplete nature of the contracts that regulate business transactions (Grossman and Hart, 1986), trading partners encourage reciprocal investments by establishing long-term business relations. Nonetheless, such investments might be interpreted as sunk costs, incentivizing opportunistic behaviors. If contractual obligations are not respected, thus capturing trading rents, *ceteris paribus* suppliers will try to leverage their monopoly power and impose higher prices.

It is thus clear that the only way to solve this problem is for judiciaries to limit such opportunistic behaviors. Each time a case is brought to court, uncertainty arises with regards to the legal issues hereby litigated (Williamson, 1985). As a consequence, a fast judiciary acts as a fundamental deterrent against economic agents' willingness to deviate from previously signed contracts. By solving legal disputes, judges produce a "judge-made" legal certainty, limiting the negative externalities deriving from opportunism. By decreasing uncertainty and making contractual obligations more likely to be performed, a functioning judiciary contributes to encourage investments and ultimately economic development.

Within this theoretical framework, international organization starting with the World Bank's Doing Business program (World Bank, 2016) and the seminal work by Djankov et al. (2003), but also the OECD (Palumbo et al., 2013) and its European counterparts (CEPEJ, 2014) have devoted a lot of attention to the measurement of courts' performance and the ways to improve it. Similar policy orientations have been supported by an equally vibrant scholarly literature that has tried to empirically estimate the positive effect of a well-functioning judiciary and a fast litigation system for economic development (Visaria, 2009; Chemin, 2009, 2012; Ippoliti et al., 2015; Mora-Sanguinetti et al., 2017; Mruk et al., 2019; Melcarne and Ramello, 2020; Liu et al., 2020; Cepec and Grajzl, 2020).

Nonetheless, these studies mostly concentrate their attention to estimate the estimation of a causal impact of judicial performance on some socio-economic outcome, usually exploiting some exogenous variation in courts' delay. Accordingly, not much has been said about what are the true determinants of courts' performances. Early literature (Buscaglia and Ulen, 1997; Botero et al., 2003; Djankov et al., 2003) claimed procedural formalism to be the most relevant driver favoring the speeding up of legal cases' resolution. As said above, these results have then vastly inspired the policy suggestions of international organizations like the World Bank: a less formalized procedure would allow courts to deliberate faster, thus ultimately supporting economic activity.

Similar positions have attracted many criticisms mainly suggesting that a faster judiciary does not necessarily imply a better justice and that improving quantitative indicators might come at the cost of an overall deterioration in the quality of the judiciary's work. In order to empirically test the hypothesis that justice delayed is actually justice denied, one must first discuss whether the performance of judiciaries (in its quantitative dimension) is in somewhat contrast with the overall quality of the "justice" delivered by courts. In other words, is there a *trade-off* between timeliness and quality of justice?

Regardless of the specific quantitative dimension of judicial performance taken into consideration (delay, productivity, clearance rate or technical efficiency), the emphasis always lays on aspects such as the number of cases solved with given resources or the timeliness of their disposition. Nothing is said about "how" such cases are dealt: are

cases solved correctly? is legal truth ascertained via judicial decisions? Is the Rule of Law respected? One might thus suggest that judicial performance and the quality of justice might not necessarily correlate positively.

Marciano et al. (2019) have discussed several potential reasons that could motivate the existence of a trade-off between quality and timeliness. The main point derives from the fact that measures of judicial performance as delay do not carry much information about the qualitative dimension of judges' work and thus the way the law is actually enforced. Accordingly, a well-performing judiciary that is able to enforce contracts in a timely way, could at the same time imply systematic violations of the rule of law, thus turning into a sort of "extractive" institution (Acemoglu and Robinson, 2012). Another theoretical motivation for a negative correlation between quality and judicial delay derives from the fact that a qualitatively good legal enforcement should make the judiciary more bound to the legal framework and thus, potentially, slower and less effective. While a theoretically ideal court should be able to solve cases almost instantly Djankov et al. (2003), physical and procedural constraints necessarily slow the work of judges. Accordingly, one might say that if reducing judicial delay is beneficial for the economy, making justice needs time and thus reduction of delay (better quantitative performance) might come at the cost of worsening the quality of justice, thus contradicting the aforementioned legal maxim.

Preliminary evidence seem to confute the existence of a trade-off between quality and timeliness (Buscaglia and Ulen, 1997; Djankov et al., 2003; Rosales-López, 2008; Dimitrova-Grajzl et al., 2012; Melcarne and Ramello, 2015; Dimitrova-Grajzl et al., 2016). The correlation between judicial performance and quality of justice seems to be positive, suggesting that countries with fast judiciaries are also the ones granting a good quality of justice. Nonetheless, more recent works (Marciano et al., 2019) suggest that once controlling for factors that could interplay with these measures, such correlation, despite remaining positive, loses all its statistical significance. As Posner (1998) claims, we might have some sort of *chicken or the egg* dilemma: a country without an established "justice" might not have the means to afford a fast judiciary, but without timely decisions, no country can afford to deliver fair justice. If causality was to run in the opposite direction as the legal maxim states, this would significantly undermine the impact of the policy suggestions made by the aforementioned international bodies.

Building on these preliminary findings, the main contribution of this work is to try to advance some further claims on the relationship between judicial performance and the quality of justice. Well aware of the difficulty of assessing causal relationships among institutional variables as correctly suggested by Klick (2010) and Helland (2016), we propose some alternative methodologies, highlighting their pros and cons, that might better approach the "ideal" first best result of isolating a causal impact of courts' delay on the quality of justice and thus try to empirically test the famous legal maxim: *justice delayed is justice denied*. Employing a new way of instrumenting judicial delay and a panel vector autoregression approach, we find statistical significant results confirming the strong positive relationship between judicial performance and the quality of justice. Despite statistical tests seem to support the validity of our approach, given the limits of this kind of research, we remain though cautious when interpreting our estimates as causal inference, hoping future literature might build upon our results to further advance this stream of institutional empirical analysis.

The remainder of the work is organized as follows. In Section 2, we describe the

two identification strategies we wish to employ in our empirical analysis, namely the instrumental variable (Section 2.1) and the panel-vector autoregression approaches (Section 2.2). In Section 3 we describe the data used in our econometric analysis, while in Section 4 we discuss the estimates thus obtained. Conclusions are drawn in Section 5.

## 2. Identification Strategy

### 2.1. IV approach

As emphasized above, our goal is to try to disentangle the relationship between courts' timeliness (proxied by judicial delay) and judicial quality. To this end, we propose two different approaches in order to limit the potential biases that might arise from measurement error and other sources of endogeneity. We are well aware that, given the nature of the institutional variables employed in our analysis and the problems related to their measurement, we cannot give definite evidence on the causal relationship under scrutiny. Nonetheless, we wish to propose two new approaches that might be employed by future literature: namely, an innovative instrumental variable for judicial delay and panel vector-autoregression (PVAR) models.

The first methodology we wish to employ in the attempt to unbundle the relation between delay and quality is the instrumental variable two stage least squares (2SLS) approach. Do prolonged judicial delays hinder judicial quality or, conversely, does deteriorating judicial quality lead to longer judicial delays? The key advantage of 2SLS approach is that the relationship can be both identified and tested through the exclusion restriction in a more rigorous sense. Provided that a variable directly unrelated to judicial quality exists, the effect of judicial delay on judicial quality can be ideally identified. By relying on a 2SLS approach, we propose the ex-ante number of judicial procedures as a plausibly exogenous source of variation in judicial delay. This approach enables us to better frame the relationship between judicial delays and quality. Deploying some of the more recent diagnostic tests, we are also able to elicit the relative strength of the identification of the nexus between delays and quality using ex-ante number of procedures as a plausibly exogenous source of variation in the quality of justice. Compared to a more traditional ordinary least squares (OLS) methodology, the 2SLS approach enables us to isolate the impact of judicial delays on judicial quality and address the simultaneous relationship between both variables. By relying on the number of judicial procedures as an ex-ante plausibly exogenous source of variation in judicial quality, we attempt to assess whether judicial delays shape judicial quality or vice versa. If the exclusion restriction holds, judicial procedures influence judicial quality only through the channel of judicial delays. If the exclusion restriction fails, the first-stage relationship should be weak along with fragile orthogonality conditions. The identification strategy consists of two steps. First, we build an endogenous model setup where the basic cross-sectional specification that takes place is:

$$Q_i = \alpha_0 + \alpha_1 \cdot Delay_i + \mathbf{X}_i' \beta + \epsilon_i \quad (1)$$

Where  $Q$  denotes judicial quality,  $Delay$  is our measure of judicial delay, and  $\mathbf{X}$  is the vector of covariates related to institutional quality. Stochastic disturbances are denoted by  $\epsilon$ . The major identification threat arises from the non-zero covariance between the

random error term and the judicial delay variable since the latter might be correlated with the omitted quality shift covariates which invokes the standard omitted variable bias. Econometrically, the non-zero covariance between judicial delay and the omitted variable is posited as  $\text{cov}(\text{Delay}_i, \epsilon_i) \neq 0$ , which implies that the standard OLS estimated model specification in Equation 1 is inconsistent.

The omitted variable bias arises from the endogeneity dilemma. While judicial delay may influence quality, the deterioration of the judicial quality may impact delay as discussed above. Hence, in the absence of the plausibly exogenous source of variation in judicial delay, the identification of our key structural parameter  $\alpha_1$  is not possible. We address the potential reverse causation between judicial delay and judicial quality by exploiting the cross-country variation in the number of judicial procedures necessary to dispose a lawsuit.

Our key identifying assumption is that the number of judicial procedures is determined ex-ante, and thus tends to influence judicial quality only indirectly through its impact on judicial delay. The corresponding first-stage specification that takes place is:

$$\text{Delay}_i = \phi_0 + \phi_1 \cdot p_i + \mathbf{X}_i' \mu + u_i \quad (2)$$

Where  $p$  denotes the number of judicial enforcement procedures. The key parameter of interest is  $\phi_1$ , where we assume that the exogeneity restriction  $\text{cov}(q_i, \text{Delay}_i) \neq 0$  and relevance criteria  $\text{cov}(p_i, Q_i) \neq 0$  are satisfied, which we further submit to rigorous empirical testing. Provided that exogeneity and relevance assumptions are satisfied, the instrumental variable should allow us to possibly isolate the impact of delay on judicial quality and overcome the inconsistencies arising from the OLS estimator.

The number of procedural steps needed to decide a case is intuitively correlated in a positive way with the time need to dispose a lawsuit. This idea has been previously tested by Spamann (2010). Using an earlier version of the same data we employ in our analysis, the author finds via first-differences regression a plausible causal impact of procedural steps on delay. Using this variable has its limits though as the number of procedures is a mere sum with equal weights of very different procedural aspects of a legal case. Thus the measure does not reflect an efficient combination of procedural steps and thus one cannot automatically conclude the policy implication that reducing the number of steps would be beneficial for cases' length, as the reduction could regard important phases of the civil procedure. Nonetheless, since most countries' procedures include a similar number of essential steps, this measure's variation derives most likely from extra steps of dubious efficiency due to a country's specific level of historical development of legal procedure (Spamann, 2010). Accordingly one might possibly claim that, differently from other possible variables, our instrument is not just a matter of socio-economic development, but more likely depends on long lasting legal traditions of procedural formalism which have barely changed over time, at least in the short run.

Djankov et al. (2003) claimed that variance in similar measures of procedural formalism largely depended on the legal origin (civil vs. common law) of a country's legal system, possibly transplanted via colonialism. Following studies relying on better data however have shown that there is no statistically significant difference in procedural complexity, between civil law and common law countries (Klerman and Mahoney, 2007; Spamann, 2010). This could in turn potentially undermine the robustness of our 2SLS analysis. If variance in procedural formalism does not derive from legal tradi-

tion, despite being a very “ossified” measure at least in the short and medium run, it could depend on other factors themselves affecting judicial quality in the long run, but not through the delay channel. For this reason we also supply in the next Section an alternative estimation strategy based on panel-vector autoregression.

## 2.2. Panel-vector autoregression approach

One potential caveat against the use of IV to unravel the effect of delay on quality is the difficulty in establishing the exclusion restriction. Most empirical institutional analyses are constrained by the scarcity of plausibly exogenous sources of variation in the judicial or institutional outcomes that are often times difficult to ascertain using IVs which might be correlated with the stochastic disturbances. This implies that many counterexamples can be offered as an alternative exogenous source of variation in the quality of justice.

One reasonable methodological alternative to determine whether judicial delay has an effect on judicial quality is to adopt a structural panel-vector autoregression approach and estimate a system of structural relationships which might allow us to better shed light on the chain of causation using conventional causality tests. One tangible advantage of the linear panel vector autoregression (panel VAR) model is to overcome the inherent limitations of the exogeneity assumption that is grounded in the 2SLS approach. In particular, judicial delay and judicial quality can be both endogenously determined and interdependent in a static and dynamic sense. When both delay and quality are simultaneously affected, the direction of causality can be tested in a straightforward manner. Provided that temporal variance exists in both variables, the natural question to ask is whether judicial delays cause quality of justice or vice versa. This implies that if the IV estimates are internally valid, a linear panel PVAR is able to discern whether judicial delays drive judicial quality rather than the other way around. Differently from other panel models as fixed effects regressions, that do not allow to control for countries’ specific time trends that are also driven by institutional change (Klick, 2010), we believe that this methodology should allow us to better approach the desired result.

More specifically, we ideally wish to examine the effect of judicial performance on judicial quality and determine whether judicial delay causes judicial quality or vice versa. To this end, we estimate the following panel vector-autoregressive (VAR) linear equations:

$$\mathbf{y}_{i,t} = \sum_{t=1}^p \hat{\mathbf{b}}_p \cdot \mathbf{y}_{i,t-p} + \hat{\pi} \cdot \mathbf{y}_{i,t-p+1} + \mathbf{X}'_{i,t} \mathbf{\Lambda} + \mathbf{u}_i + \mathbf{e}_{i,t} \quad (3)$$

Where  $\mathbf{y}$  is a  $1 \times k$  vector of dependent variables which consist of the judicial delay or quality variable,  $\mathbf{X}$  captures the set of structural confounders such as the level of political institutions,  $\mathbf{u}_i$  and  $\mathbf{e}_{i,t}$  are vectors of dependent variable-specific fixed-effects and idiosyncratic errors. The  $k \times k$  matrix  $\hat{\mathbf{b}}$  and the  $l \times k$  matrix  $\mathbf{\Lambda}$  are parameters to be estimated. Equation 3 represents the k-covariate PVAR model specification of order  $p$  with panel-specific country-fixed effects represented by the system of linear equations for  $i \in \{1, 2, \dots, N\}$  countries and  $t \in \{1, 2, \dots, T_i\}$  years.

The structural parameters in Equation 3 can be estimated jointly with the fixed effects estimator or independently of the fixed-effects transformation using equation-by-equation OLS estimator. We prefer to estimate the structural relationship between

judicial quality and timeliness using fixed-effects transformation to ensure that the effects are not confounded by the unobserved heterogeneity. The traditional approach to estimate structural PVAR specification employs the set of lagged dependent variables. However, lagged estimates would be biased (Nickell, 1981) even in the presence of large  $N$ . As  $T$  becomes large, the bias approaches zero, but Judson and Owen (1999) find significant bias even when, for example,  $T = 30$ . To overcome these issues in our panel with a large  $N$  and relatively small  $T$ , we use GMM estimation to obtain consistent estimates of Equation 3 with a fixed  $T$  and large  $N$  as previously suggested by Kiviet (1995), Bun and Carree (2005), Everaert and Pozzi (2007), and Canova and Ciccarelli (2013) among others.

Our key assumption is that the transitory shocks to judicial delay and quality are serially uncorrelated, that is:  $E[e_{i,t}] = 0$ ,  $[e'_{i,t}e_{i,t}] = \sum$  and  $E[e'_{i,t}e_{i,t}] = 0$  for all  $t > p$ . This effectively ensures that first-difference transformation of Equation 3 may be consistently estimated by instrumenting the lagged differences with differences and levels of  $y_{i,t}$  from earlier periods as suggested by Anderson and Hsiao (1982). If some  $y_{i,t-1}$  is not available, then the FD transformation at time  $t$  and  $t - 1$  is likewise missing. Given fixed  $T$ , we set the number of lags for set of dependent variables to  $p = 2$ , as this is the necessary time period where each panel is observed to yield reasonably unbiased estimates. As the lag order of the PVAR gets larger, more realizations are required to estimate the structural parameters consistently. For instance, with the second-order lag, instruments in levels require that at least  $T_i \geq 5$  realizations are observed for each panel. 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 realizations are necessary to produce instruments in levels and past realizations are not included in this transformation, the instruments remain valid because  $T_i \geq 4$ .

Including a longer set of lags of instrumental variables would clearly improve the efficiency of our estimates but observations may be sharply reduced, especially with unbalanced panels or with missing observations. Following Holtz-Eakin et al. (1988), we build the set of moment conditions using observed realizations, and substitute missing observations with zero assuming that the IVs are directly uncorrelated with the transitory shocks. This permits equation-by-equation consistent GMM estimates of the structural parameters with a clear efficiency gain<sup>1</sup>.

### 3. Data

Table 1 presents the key descriptive statistics for the data employed. Our sample consists of 171 countries observed for the period 2003-2016. Panel A reports the means and standard deviations for the judicial quality variables. In order to guarantee the stability of our estimates across different specifications, we employ two different versions

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<sup>1</sup>The panel vector autoregression approach can be implemented in a straightforward way in Stata by using “pvar” command. See Abrigo and Love (2016) for further clarification of the software code. Sigmund and Ferstl (2019) provide a user-friendly introduction to the implementation of PVAR approach in the R package.

of our dependent variable of interest: a relatively “objective” metric as the Judicial Quality Index (JQI)<sup>2</sup> provided by the World Bank’s Doing Business program (World Bank, 2016), and the more canonical Rule of Law Index (RoL) provided by the World Bank’s Worldwide Governance Indicators project (Kaufmann et al., 2011). The lowest level of the JQI is observed in Iraq while the countries with the highest level are Australia, United Kingdom and Singapore. For the RoL, we observe the lowest in-sample value in Venezuela while the highest levels are observed in Finland, Norway and Sweden.

Our judicial delay (JD) variable is extracted from the World Bank’s Doing Business Enforcing Contract’s “time” metric. The mean value of JD in our sample is 636 days, ranging between 164 days (in Singapore) and 1,715 days (in Suriname). Panel C reports the descriptive statistics for the covariates we use as controls in our empirical analysis, while Panel D presents the key descriptive statistics for our IV, the number of contract enforcement procedures.

Table 1: Outcome-Level and Covariate-Level Descriptive Statistics

	Mean	StD	Min	Max
<i>Panel A: Judicial Quality Variables</i>				
Judicial Quality Index	8.37	3.1	2 (Iraq)	15.5 (Australia)
Rule of Law Index	-0.017	0.96	-2.17 (Venezuela)	2.04 (Sweden)
<i>Panel B: Judicial Timeliness Variable</i>				
Judicial Delay (days)	636.48	301.65	164 (Singapore)	1715 (Suriname)
<i>Panel C: Control Variables</i>				
Trade GDP Ratio	93.4	56.7	24.8 (Brazil)	437.5 (Honk Kong)
Civil Law Legal Origin	0.71	0.49	0 (France)	1 (USA)
Population Growth	1.45	1.35	-1.76 (Syria)	8.09 (Oman)
Malaria Index	0.43	0.49	0 (Luxembourg)	1 (Belize)
Oil Producer	0.07	0.25	0 (Germany)	1 (Iran)
PolityIV	4.04	6.21	-10 (Rwanda)	10 (USA)
<i>Panel D: Instrumental Variables</i>				
# Contract Enforcement Procedures	37.94	6.50	21 (Ireland)	55 (Syria)

<sup>2</sup>This Index measures whether a judiciary adopts a series of good practices in its court system in order to guarantee high qualitative standards. For a description of the methodology adopted to measure this index refer to <https://www.doingbusiness.org/en/methodology/enforcing-contracts>.

Figure 1 gives us a graphical representation of the correlation between judicial delay and our two measures of judicial quality across a large cross-section of 171 countries. In order to better visualize judicial performance (in terms of timeliness) increasing from left to right, we reverse the scale of judicial delay. We also add reference lines for the median values of our variables of interests. This allows us to better visualize the data and sort countries in clusters thanks to the four quadrants thus created.

The north-east quadrant captures the cluster of “model” countries with low judicial delays and high judicial quality and rule of law. These countries are generally characterized by high state capacity in enforcing law and order, and a well-functioning judiciary with a quick turnaround period. On the contrary, the south-west quadrant comprises the worst scenario: countries with a poorly functioning judiciary characterized by relatively low respect of the Rule of law and also prolonged delays. These countries are primarily from the developing regions in Sub-Saharan Africa (Angola, Liberia), Middle East (Egypt, Syria) and Latin America (Guatemala, Honduras). They suffer from weak state capacity, widespread judicial delays and low-quality judiciary. The other two quadrants present a slightly more complicated situation. Here it seems that judicial performance (in terms of low delays) is not accompanied by high quality, thus partially confuting our legal maxim of interest. The north-west quadrant captures the cluster of countries with a relatively high-quality judiciary and robust rule of law but with prolonged and widespread judicial delays. Some of these countries are from Southern Europe (Greece, Italy, Slovenia) while others are from Latin America (Brazil, Colombia) or Middle East (Israel). By contrast, the south-east quadrant represents on the contrary the group of countries characterized possibly by what we defined above as an “extractive” judiciary: a low-quality but yet fast justice.

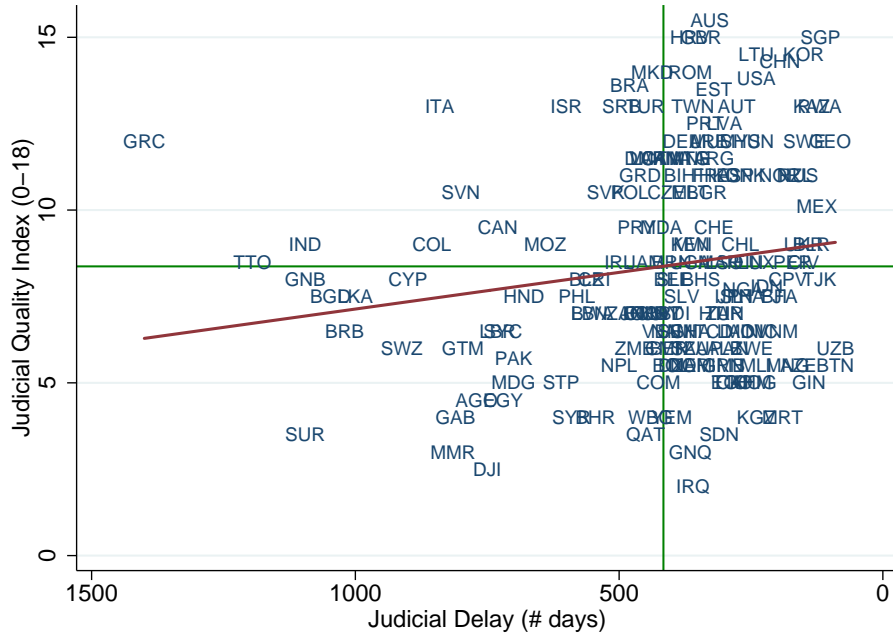
Figure 1 plots the aggregate correlation between judicial delay and our two measures of quality of justice. Consistently with previous findings (Marciano et al., 2019), the descriptive evidence suggests a positive relationship (statistically significant at 1% level) between our two measures of quality and judicial performance. This kind of simple cross-country comparison inevitably reveals the complexity of the relationship between judicial quality and delay and the need for a deeper empirical investigation.

## 4. Empirical Analysis

### 4.1. IV Results

Is justice delayed justice denied? The descriptive evidence so far merely documents a strong positive relationship between judicial performance and quality. We are well aware of the fact that, because of issues of measurement error and omitted variable bias, providing a definitive answer when dealing with similar institutional variables is far from easy. Accordingly, we now try to contribute to the scholarly debate by suggesting two new ways to approach this problem. First, we show the results of our 2SLS analysis using procedural formalism as an instrument. In section 4.2 we show that our IV estimates seem to be confirmed also by an alternative panel-vector autoregression approach.

In Table 2, we report the IV estimated interrelationship between our two measures of judicial quality (JQI and RoL) and judicial delay. Given the fact that our instrumental variable is basically time-invariant, we decided to opt for a cross-sectional analysis, using country-average values for all other time variant variables. Columns (1) and



(a) Judicial Quality Index and Judicial Delay



(b) Rule of Law and Judicial Delay

Figure 1: Judicial Delay-Quality Correlation. Green vertical and horizontal lines represent the median value in Judicial Delay and the Judicial Quality variables. The red lines represent a linear fit of an OLS regression of the Judicial Quality variable on Judicial Delay. The Judicial Delay variable is extracted from the World Bank's Doing Business Enforcing Contract's "time" metric and measures the average number of days to conclude a lawsuit in a country. The Judicial Quality Index in Figure 1a is provided by the World Bank's Doing Business program. The Rule of Law Index in Figure 1b is provided by the World Bank's Worldwide Governance Indicators project.

Table 2: Judicial Delay-Quality Trade-Off (full sample)

		Judicial Quality			
		(1)	(2)	(3)	(4)
		Judicial Quality Index		Rule of Law	
Panel A: Endogenous Setup					
	Judicial Delay	-.014*** (.003)	-.006*** (.002)	-.005*** (.001)	-.003*** (.001)
Panel B: First Stage OLS Setup for Judicial Delay					
	# Enforcement procedures	12.671*** (2.961)	12.848*** (3.611)	12.671*** (2.961)	12.954*** (3.610)
	Covariates	✗	✓	✗	✓
	First-Stage Angrist-Pischke F-Test ( $p$ -value)	0.000	0.000	0.000	0.002
	Stock-Wright Orthogonality Test ( $p$ -value)	0.000	0.042	0.000	0.000
	Cragg-Donald Weak Identification Test	13.65	10.38	13.65	10.94
	Kleibergen-Paap Underid. Test ( $p$ -value)	0.000	0.000	0.000	0.000
	# countries	171	163	171	164
<p>Panel A displays the endogenous setup with the structural interrelationship between judicial delay and our two judicial quality variables. Panel B presents the first-stage OLS relationship for the underlying judicial delay variable. The null hypothesis of the Angrist-Pischke test is that the underlying endogenous variable is not identified with the set of instrumental variables. The null hypothesis of the Stock-Wright orthogonality test is that the coefficients of the endogenous variables in the structural relationship are jointly equal to zero. The null hypothesis of the Kleibergen-Paap test is that the structural relationship is underidentified in which case the rank condition fails. The null hypothesis of the the Cragg-Donald weak identification is that is the number of enforcement procedures does not provide sufficiently strong identification properties for a causal interpretation of the relationship between judicial delay and judicial quality. Standard errors are adjusted against serially correlated stochastic disturbances and arbitrary heteroskedasticity using Huber-White sandwich estimator for the underlying empirical distribution function. Asterisks denote statistically significant sample coefficients at 10% (*), 5% (**) and 1% (***), respectively.</p>					

(2) of Table 2 explore the plausible effect of judicial delays on JQI. First-stage OLS evidence in Panel B clearly suggests that a greater number of procedures to enforce a contract is associated with markedly higher judicial delays. Pointwise, our estimates imply that each additional procedural step is associated with an increase in the period of contract enforcement of about 12 days, also when controlling for other covariates traditionally employed to control other channels affecting institutional quality. In the second stage reported in Panel A, our estimates show that expanding the contract enforcement time by 1 percent is associated with a drop in the judicial quality index between 1.4 and 0.6 basis points, when introducing controls. Columns (3) and (4) replace the JQI index with the RoL index as an alternative dependent variable, in order to provide equally robust estimates across different specifications. The estimates from the first stage and second stage confirm our findings in columns (1) and (2).

For the sake of comparison, Table A1 and Table A2 in Appendix Appendix A present, respectively, the second-stage and first-stage analyses including the coefficients for all covariates included in our models, that is the estimated models in columns (2) and (4) of Table 2. It is worth highlighting how, from Table A2, in the first stage, among all considered covariates, our instrumental variable appears to be the only significant determinant of judicial delay.

Notice that according to statistical tests, our IV appears to be a relevant and possibly exogenous instrumental variable that tackles the simultaneity bias in our timeliness-quality relationship plausibly well. The  $p$ -value on the F-test on the excluded instrument (Angrist and Pischke, 2008) is consistently low and within 1% bound, which largely confirms the relevance of the numbers of procedural steps in accounting for the cross-country differences in judicial delay.

In addition, the  $p$ -values from the Kleibergen and Paap (2006) test are consistently within the 1% bound. This allows us to consistently reject the null hypothesis that the matrix of reduced-form coefficients is under-identified and thus conclude that our identification of the delay-quality structural relationships does not necessitate additional or alternative instrumental variables. Lastly, we test the joint significance of endogenous covariates in the main structural equation using a test proposed by Stock and Wright (2000). Notice that the orthogonality conditions in specifications across columns (1) through (4) are very strong with the corresponding  $p$ -values within 5% bound.

If one were to consider our IV as a plausible source of exogenous variation for judicial delay, one could interpret the estimates in Table 2 as empirical evidence of the causal impact of judicial delay on the quality of justice, thus the legal maxim: justice delayed is justice denied. Nonetheless, we are well aware of the limits connected to using institutional variables as instruments, and thus we remain cautious about the possibility of interpreting causality from the estimates in Table 2. For this reason we also try to explore an alternative and new identification strategy relying on dynamic panel estimation.

#### *4.2. Panel-vector autoregression results*

Table 3 reports the structural panel-vector autoregressive (PVAR) estimates of the judicial timeliness-quality relationship. The key advantage of the PVAR approach is the possibility of attempting to disentangle the causal relationships from the set of structural equations and determine whether the causes happen before the consequences. In the judicial quality-timeliness context, this boils down to trying to better

approach an understanding of whether judicial delay causes judicial quality or vice versa. Compared to the traditional IV approach, PVAR empirical strategy imposes no exclusion restriction on the structural relationship and thus fends off any concern regarding the validity of the exogeneity condition. Unfortunately, the World Bank does not provide measures of its JQI metric for years prior to 2016, thus in this section we rely only on our Rule of Law index (RoL) as a measure of the quality of justice.

Column (1) of Table 3 reports the full-sample specification using the RoL index

Table 3: Structural panel-vector autoregressive estimates of judicial delay and quality

	(1)	(2)	(3)	(4)
Panel A: Structural panel estimates				
	Full Sample		Only Civil law	
Dependent Variable	Rule of Law	Judicial Delay	Rule of Law	Judicial Delay
Rule of law (t-1)	.952*** (.133)	28.174 (35.778)	1.071*** (.122)	36.671 (32.662)
Judicial delay (t-1)	-.001*** (.004)	.772*** (.130)	-.0007*** (.0002)	.822*** (.100)
PolityIV (t-1)	-.013 (.009)	-2.622 (2.407)	-.009 (.011)	-.868 (2.903)
# Observations	1,490	1,490	1,280	1,280
# countries	149	149	128	128
Eigenvalue stability condition	✓	✓	✓	✓
Panel B: Granger-Wald causality tests (p-values)				
Excluded variable:				
Judicial delay	(0.006)		(0.007)	
Rule of law		(0.431)	(0.153)	
Polity	(0.176)	(0.276)	(0.423)	(0.765)
Panel A reports the structural panel estimates whilst Panel B reports Granger-Wald causality tests for the excluded variable in question. The null hypothesis of the Granger-Wald causality test is that the selected excluded variable does not Granger-cause the equation-level variable. The alternative hypothesis is that excluded variable does Granger-cause the equation variable. When the null hypothesis that the selected variable does not cause the dependent variable is rejected but not vice versa, the p-values indicate whether or not the simultaneous relationship between both variables exists or not. 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.				

as a dependent variable. Evidence indicates a high degree of persistence given that the coefficient on the lagged rule of law is close to unity and statistically significant at 1%. As theoretically expected, the coefficient on the judicial delays is negative and significant at 1%, suggesting that greater judicial delays are associated with a deterioration of the rule of law. Against this backdrop, Panel B reports the Granger-Wald causality test. In column (1), the estimates provide the evidence in support of rejecting the null hypothesis that judicial delays do not Granger-cause judicial quality (i.e. rule of law). In particular, the  $p$ -value on the judicial delays as the excluded variable is 0.006 which suggests that the null hypothesis can be comfortably rejected at conventional

significance levels. Column (2) reports the PVAR estimates using judicial delay as a dependent variable. Despite an intuitively high degree of persistence in judicial delays, the coefficient on the lagged rule of law index is not statistically significant even at artificially high thresholds. This is further bolstered in Panel B where the  $p$ -value on the rule of law index as the excluded variable is 0.43 and thus suggests that the null hypothesis of no causal effect of judicial quality on judicial delay cannot be rejected. The estimates in column (1) and (2) both satisfy the eigenvalue stability condition since both real and imaginary eigenvalues lie inside the unit circle, providing the necessary basis for the identification of structural relationship of interest.

Notice that the estimated specifications in column (1) and (2) report both a lagged rule of law and lagged judicial delay variables. Including both lagged terms allows us to compare the relative magnitudes of the past delay and quality and determine the size of each specific effect. This implies that past judicial quality and rule of law matter greatly for their current levels. The comparison of effects is informative in distinguishing between persistence of past quality and delay and the potential effect of delay on quality or vice versa. The comparison of both lagged terms also reveals that past rule of law does not lead to discernible changes in judicial delays. The comparison of both lagged terms also reveals that past rule of law does not lead to discernible changes in judicial delays. On the other hand, as demonstrated in column (1), past delays shape the contemporaneous rule of law even after lagged rule of law is controlled for which has immediate implications for the significance of the delay as the excluded variable in explaining judicial quality.

In columns (3) and (4), we re-estimate the two structural equations by dropping common law jurisdictions off the full sample. The evidence unequivocally suggests that past judicial delays seem to possibly predict future judicial quality (proxied by the rule of law index) while past judicial quality does not predict future judicial delays. In column (3), the coefficient on lagged rule of law is similarly persistent as in column (1) whereas, as expected, the coefficient on lagged delays is negative and statistically significant at 1%. When the causal relationships are tested in Panel B, the  $p$ -value on lagged delay is 0.007 which implies that the null hypothesis of absent causal effect can be safely rejected. By contrast, in column (4) the causality test does not lend a strong support for the claim that past rule of law index predicts future judicial delays given that the  $p$ -value on the rule of law as the excluded variable is above 0.10, respectively.

In order to further test the robustness of our estimates we have included in all our specifications a potential confounder variable as the level of democracy measured by the PolityIV metric as an additional explanatory variable. In all specifications, the relationship between judicial delays and rule of law does not appear to be tainted by the confounding influence of the level of democracy given that political institutions per se may affect both judicial delays and the rule of law. In particular, the  $p$ -value on the PolityIV as a determinant variable is consistently high and does not support the notion of political institutions being a decisive factor in the relationship between judicial delay and quality.

At the general level, the evidence from Table 3 seems to confirm our previous IV estimations. Furthermore, if one would assume that our approach correctly identifies the theoretical causal effects of interest, our estimates would suggest not only that justice delayed appears to be justice denied, but also that respecting higher standards of quality does not seem to slow down the functioning of the judiciary. However, as

we argued above, we are conscious of the limits of the evidence we supply and thus equally remain prudent about interpreting the estimates from Table 3 as definitive evidence of the causal impact of judicial performance (in terms of delay) on the quality of justice. We believe that with respect to previous works suggesting a positive correlation between these two measures (Buscaglia and Ulen, 1997; Djankov et al., 2003; Marciano et al., 2019), the present work has supplied some new insights, namely a new instrument and a panel approach, to further dig into this institutional relationship, hoping that future literature might build upon them to further develop this stream of institutional empirical analysis.

## 5. Concluding Remarks

Is justice delayed justice denied? This well-known legal maxim, according to some attributable to British Prime Minister William E. Gladstone (1809-1898), has found various practical applications in a number of countries' legislations. From article 6 of the European Convention on Human Rights to the US federal statutes regulating the consequences for "slow" judges (Title 28, §476(a)(3)). Far from being confined to a generic legal principle, this maxim has also inspired the reforms attempting to improve the performance of national judiciaries all around the world. In fact, the empirical findings showing that faster judiciaries are beneficial for economic development inspired international organization to promote some sort of "one size fits all" policy suggestion in favor of streamlining procedure and speeding up the resolution of lawsuits. However, the same literature promoting the importance of fast judiciaries has only vaguely attempted to tackle the potential drawbacks of a quicker case resolution in terms of the possible deteriorations of the overall quality of the justice system.

The present work has attempted to contribute to this literature by trying to address directly this issue from an empirical perspective. We have motivated the reasons why it is important for the scholarly debate to further disentangle the interplay between quality of justice and judicial performance. Then we have proposed two alternative identification strategies that might, to a certain extent, better cope with the intrinsic limits that the empirical analysis of institutional variables inevitably has to deal with. We use the number of procedural steps necessary to solve a case as an innovative instrumental variable for capturing plausibly exogenous variation of judicial delay and a panel vector autoregressive approach. Our estimates confirm the existence of a significant negative relationship between judicial delay and the quality of justice, thus suggesting that in countries where justice is delayed, justice is also denied. However, as we already emphasized above, we remain cautious with respect to interpreting our results in line with a causal effect. Accordingly, we cannot give a definitive answer to the question raised above. Nonetheless, we hope that building upon our results future scholarship might be able to further refine the empirical analysis and be able to ascertain the causality running from judicial performance to the quality of justice.

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## References

## Appendix A. Appendix: Additional Tables

Table A1: Second stage with all covariates

Dependent Variable	Judicial Quality	
	(1) Judicial Quality Index	(2) Rule of Law
Panel A: Endogenous Setup		
Judicial Delay	-.006*** (.002)	-.003*** (.001)
Panel B: Second Stage Structural Controls		
Trade GDP Ratio	-1.006* (.562)	-.098 (.193)
Civil Law Legal Origin	-.408 (.452)	-.158 (.186)
Population Growth	-.771*** (.189)	-.048 (.058)
Malaria Index	-.256 (.603)	-.056 (.246)
Oil Producer	-1.280 (.967)	-.305 (-.433)
Infant Mortality	-.033** (.014)	-.018*** (.006)
First-Stage Angrist-Pischke F-Test ( $p$ -value)	0.000	0.002
Stock-Wright Orthogonality Test ( $p$ -value)	0.042	0.000
Cragg-Donald Weak Identification Test	10.38	10.94
Kleibergen-Paap Underid. Test ( $p$ -value)	0.000	0.000
# countries	163	164
Standard errors are adjusted against serially correlated stochastic disturbances and arbitrary heteroskedasticity using Huber-White sandwich estimator for the underlying empirical distribution function. Asterisks denote statistically significant sample coefficients at 10% (*), 5% (**) and 1% (***), respectively.		

Table A2: First stage with all covariates

Dependent Variable	(1)	(2)
Judicial Delay		
Panel A: First-Stage IV Parameter Estimates		
# Enforcement Procedures	12.671***	12.954***
	(2.961)	(3.610)
Panel B: First-Stage Covariate Estimates		
Trade GDP Ratio	-.562	-.562
	(.505)	(.505)
Civil Law Legal Origin	.139	.139
	(.478)	(.478)
Population Growth	-.248	-.248
	(.162)	(.162)
Malaria Index	.842	.842
	(.619)	(.619)
Oil Producer	-.237	-.237
	(.163)	(.163)
Infant Mortality	-.003	-.003
	(.148)	(.148)
First-Stage Angrist-Pischke F-Test ( $p$ -value)	0.000	0.002
Stock-Wright Orthogonality Test ( $p$ -value)	0.042	0.000
Cragg-Donald Weak Identification Test	10.38	10.94
Kleibergen-Paap Underid. Test ( $p$ -value)	0.000	0.000
# countries	163	164
Standard errors are adjusted against serially correlated stochastic disturbances and arbitrary heteroskedasticity using Huber-White sandwich estimator for the underlying empirical distribution function. Asterisks denote statistically significant sample coefficients at 10% (*), 5% (**) and 1% (***), respectively.		